
SPATIAL ASPECTS OF THE LABOR MARKET

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Abstract

Spatial Aspects of the Labor Market

by Franziska Assmann (née Hawranek)

This thesis discusses spatial aspects of the labor market. I analyze the interplay of spatial location of residential location and the labor market as well as a policy designed to affect individuals' residential distribution throughout space.

Chapter 2 addresses the question whether migrants earn less than Germans due to specific residential locations. I examine the role of residential segregation on wage differentials using panel and cross sectional regression analysis. In contrast to previous findings on the topic, I find no negative correlation of migrants' concentration in city districts with individual wages. Rather, the correlation is slightly positive. A possible driver for this positive relationship are networks based on residential locations which facilitate the integration into the German labor market. Chapter 3 analyzes the existence of informal job market networks based on residential location networks. For our analysis we use a geo-coded employer-employee data set consisting of the universe of workers in the Rhine-Ruhr metropolitan area in 2008. We show that living in the same neighborhood (defined as a 500×500m grid cell) increases the probability of working in the same firm by 30%. The finding is robust to several other specifications such as reverse causality, sorting and clustering close to central business districts.

In chapter 4 I analyze how a spatial policy affects outcomes on the labor market. Namely, we analyze how a partly withdrawal of commuting subsidies affects individual wages. Using a large and unique geo-coded data set including exact route commuting distances, we find that net wage losses are only compensated to a small extent. While the policy design before the withdrawal is regressive, the introduction of a lower bound on commuting subsidies leads to a more equal distribution of tax savings.

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Contents

Acknowledgements	v
Contents	vi
List of Figures	ix
List of Tables	xi
1 Introduction	1
2 Residential Segregation of Immigrants in Germany	9
2.1 Introduction	9
2.2 Literature Review	10
2.2.1 Measuring Segregation	10
2.2.2 Consequences of Residential Segregation	11
2.3 Data	13
2.3.1 Descriptive Statistics	14
2.4 Empirical Analysis	16
2.4.1 Fixed Effects Estimation	16
2.4.2 Intercity Approach	18
2.5 Conclusion	21
3 Job referrals based on residential location networks	25
3.1 Introduction	25
3.2 Literature Review on Neighborhood and Network Effects	27
3.3 Data	30
3.4 Empirical Design	34
3.5 Results	36
3.6 Robustness	39
3.6.1 Reverse Causality	40
3.6.2 Random Reassignment to Jobs	43
3.6.3 Firm size effects	44
3.6.4 Commuting	46
3.7 Discussion	49
4 The Distributional Effect of Commuting Subsidies	53
4.1 Introduction	53
4.2 The Reform of Commuting Subsidies in Germany between 2004 and 2009	56

4.3	Data and Descriptives	58
4.3.1	Data	58
4.3.2	Descriptives	60
4.4	Commuting Subsidies and Gross Wage Compensation	62
4.4.1	Empirical Approach	62
4.4.2	Results	64
4.5	The Distribution of Tax Savings across Worker Groups	70
4.5.1	Distribution between Wage Groups	72
4.5.2	Distribution between Regions	74
4.6	Discussion	77
5	Conclusion	79
A	Appendix Residential Segregation of Immigrants in Germany	85
B	Appendix Job Referrals Based on Residential Location Networks	89
C	The Distribution of Commuting Subsidies	95
	Bibliography	99

List of Figures

2.1	Mean Earnings for Germans and Types of Foreigners for 1998-2007 on quarterly basis	14
3.1	Defining Neighborhood by a regular Grid	30
3.2	Employees in 500×500m grid cells in the Rhine-Ruhr metropolitan area .	32
4.1	Policy Reform and Classification of Treatment and Control Group	57
4.2	Distribution of Commuting Distances	60
4.3	Gross Wage Growth by Treatment Status	66
4.4	Illustration of Robustness Checks	67
4.5	Tax Deductions Before and After the Reform, by Gross Wage	72
4.6	Tax Savings Before and After the Reform, by Gross Wage	73
4.7	Tax Deductions and Tax Savings Before and After the Reform, by Regional Density	75
B.1	Rhein-Ruhr Metropolitan Area	92
B.2	Size distribution of neighborhoods and super-neighborhoods	92

List of Tables

2.1	Descriptive Statistics on Migrant Segregation, 2007	15
2.2	Descriptive Statistics Germans vs. Immigrants, June 2007	16
2.3	Fixed Effects Panel Estimation	17
2.4	Income Segregation and Educational Exposure	19
2.5	Intercity Cross Sectional Estimation	20
3.1	Group Sizes in Population and Sample	33
3.2	Estimation of Referral Effects	37
3.3	Referral Effects amongst Job Movers and Residential Stayers	41
3.4	Baseline Estimation for artificial Workplaces	44
3.5	Firm Size Effects for Referrals to a Neighborhood	44
3.6	Firm Size Effects for Referrals to a Firm	45
3.7	Baseline Estimation excluding Short Distance Commuters	46
3.8	Referral Effects including Public Transportation Stations	48
4.1	Worker Characteristics by Treatment Status	61
4.2	Gross Wage Adjustments as Response to the Reform of Commuting Subsidies	65
4.3	Capitalization of Commuting Subsidies in Land Prices	71
4.4	Decomposition of Theil Index of Tax Savings, by Region Type	76
A.1	Classification of Foreigner Types	85
A.2	Foreigners' Nationalities conducted from BAP, wave 40	88
B.2	Estimation of Heterogenous Referral Effects, Full Output	89
B.1	Correlation between individual and average characteristics across neighbors	93

Chapter 1

Introduction

One strand of economic literature suggests that due to new technologies and globalization, space and its associated limitations for our economy are part of the past. Another strand of literature emphasizes the ongoing importance of localities and even speaks of “glocalization” (Robertson, 1995). As long as people work and live in different locations and markets respond to the attributes of their location, it is important to study the spatial implications for the labor market (Bartik and Randall, 2006). In case of job search, this is most obvious. Analyzing labor markets and their regional aspects incorporates the interaction between peoples’ residential location as well as their job location on the one hand. On the other hand, institutions and characteristics generate or reinforce this spatial distribution of households and firms (Bartik and Randall, 2006). Especially to design policies on the optimal spatial distribution it is crucial to understand the influence and mechanisms behind people’s distribution on their labor market outcomes.

In this thesis, I discuss all of these three key aspects which combine regional and labor market economics: First, I analyze how residential location affects labor market outcomes, namely how residential segregation is related to wage differentials of migrants and Germans. Second, I investigate labor market networks based on residential location as a mechanism which can lead to the aforementioned interplay of residential location and labor market outcomes. Third, I evaluate a policy designed to affect the spatial distribution of people throughout space: A commuting subsidy.

In Chapter 2¹ I analyze the relationship of migrants’ residential segregation on wages in German cities. Cutler and Glaeser (1997) show that blacks residing in highly segregated neighborhoods are worse off in terms of educational attainment, employment and single parenthood. They find, however, that controlling for exposure to college-educated neighbors explains half of the impact on residential segregation. Collins and Margo (2000) show that this has not always been the case. Using Census data from 1940 to 1980,

¹Chapter 2 consists of Hawranek (2014).

they find that the negative effects of segregation on black's socioeconomic and economic outcomes emerge only in the beginning of 1970, whereas there is no effect before that. Cutler et al. (2008) investigate the effect of segregation on educational and economic outcomes of young immigrants to the United States. They find a significant negative effect of neighborhood's segregation level on earnings. As group share on MSA level has a c.p. positive effect, the authors conclude that only an increase in tract level concentration not related to the overall MSA share leads to negative economic outcomes.

Glitz (2014) analyzes segregation of ethnic minorities in Germany both in workplaces and residential location from 1975 to 2008. His results suggest that segregation is rather stable in Germany but more pronounced for low-educated people. Additionally he shows that the longer migrants stay in Germany as their host country, the less they tend to work in segregated workplaces. In contrast to my work, he analyzes Germany as a whole and uses municipalities as smallest geographical unit, as opposed to analyzing particular cities. Glitz finds an insignificant negative relationship between residential segregation indices (computed for different ethnic groups of migrants) and average monthly income. To examine this ambiguous relationship of residential segregation on wages in Germany, I use individual data for a 2% random sample of German employees subject to social security. The data extend from 1998 to 2007 on a quarterly basis for five big German cities (Berlin, Hamburg, Frankfurt/Main, Munich and Stuttgart). I apply a panel fixed effects model in the virtue of Cutler and Glaeser (1997) to determine the effect of high concentration of foreigners on wages. I find a positive correlation between the proportion of foreigners in one's neighborhood on wages but a negative wage effect of being foreigner as opposed to German. Second, I apply a city level cross sectional estimation additionally including measures for educational exposure and segregation by income as suggested by Cutler et al. (2008). I test the hypothesis that the latter two effects lead to the main variation in wages as opposed to residential segregation. The hypothesis can be rejected, as my results suggest that educational exposure and income segregation do not significantly influence foreigners' earnings.

In summary, the empirical results suggest a positive correlation of migrant concentration in residential districts and wages. Foreigners hence seem to benefit from higher migrant concentrations in their districts or cities, respectively. There are two main explanations to this finding: First, the correlation could be driven by network effects on the basis of residential locations, which help foreigners to find a (well-paid) job. Second, there could be a correlation in unobservables, which drive both individual incomes and residential location. The results are likely to be biased by self-selection, which is not accounted for properly in chapter 2.

In addition to the econometric concerns, the definition of neighborhood used in chapter 2 is very broad due to data restrictions and privacy regulations: To observe effects from living in a specific area, the definition of this district is crucial. As the districts in

the sample encompass very different areas like “Berlin Mitte” but also “Munich City”, the classification within cities varies strongly in its acuteness and extend. Results can therefore only give a hint on the importance of inner city inequalities. This emphasizes the importance of data with precise neighborhood definitions and exact geographical information.

Although the evidence of chapter 2 is not completely convincing, Edin et al. (2003) also find that immigrants benefit from living in ethnic enclaves using Swedish data on a policy which randomly assigned migrants to counties all over Sweden. Especially low-skilled migrants experience wage increases which are even higher when exposed to higher skilled migrants. They conclude that these results can be explained by job search networks. Chapter 3² examines the existence of such labor market networks based on residential location. We use a novel data set covering geocoded record data for the entire working population (liable to social security) and the corresponding establishments. Data on the exact residential location enables us to apply a precise definition of neighborhoods, defined by grid cells of 500m×500m. This size is sufficiently small to refer on social interaction.

We use a well-established approach for approximation of a local network effect: We estimate the propensity of two individuals to work at the same place when residing in the same neighborhood. We apply a linear probability model (LPM), and compare the propensity of two individuals residing in adjacent neighborhoods, conditional on a super-neighborhood fixed effect (where super-neighborhoods are all adjacent neighborhood grid cells). The empirical design follows Bayer et al. (2008) and our results are very similar: Bayer et al. (2008) find that sharing the same immediate neighborhood raises the propensity to work together by 0.12 percentage points, whereas the effect is 0.14 percentage points in our case. This translates into a relative increase in the probability of working in the same neighborhood of 8%. When estimating the propensity to work in the same firm, we even find an increase in probability of about 30%.

We conduct a number of robustness checks and rule out several alternative explanations for this propensity effect. In particular, we test for a reverse direction of housing referrals amongst coworkers and the spurious correlation due to the geography of workplaces and transportation infrastructure. We therefore explicitly address the main caveats of chapter 2, the inaccuracy of neighborhood definition and the potential bias due to self-selection.

We differentiate job referral effects by characteristics such as education, industry, nationality or age groups. The effects differ especially by ethnicity: Compared to Germans, the propensity to work together is highly increased, in particular for immigrants from new EU countries but also from the former guest-worker countries Spain and Italy. This result reinforces the interpretation of chapter 2, where I conclude that migrants’ wages

²Chapter 3 is based on Hawranek and Schanne (2015).

increase with a higher concentration of other migrants in their neighborhood.

Chapter 3 corrects for the main short comings of chapter 2: First, I use geocoded record data with small grid cells as neighborhood definitions. In this case, comparison is possible as all neighborhoods have equal size which is small enough to actually capture social interaction and therefore allow for inference from residential location on individual labor market outcomes.

We show that residence based networks play an important role, especially for low skilled and minority workers. Moreover, the effect of residence based networks can have important implications for spatially based policies: If networks for an informal job market exist in residential locations, they can potentially be used for unemployment policies and generate local spillovers.

If it is the case that people's place of residence affects their labor market outcomes, there is an incentive for political decision makers to alter the distribution of people. One possibility to do so is via urban policies (urban renewal, investment in transportation infrastructure etc.), another way is to attempt to influence the relation of where people work and live, namely commuting. The rationale to subsidize commuting in most countries is to increase the efficiency and equity of the labor market. Subsidizing commuting hence is a means to increase the search radius of workers and therefore to enable matches in the labor market that might have failed otherwise.

In chapter 4³ we analyze distributional effects of commuting subsidies, using a large scale policy change in combination with geo-referenced employer-employee data. We employ a difference-in-difference approach to analyze whether a partial withdrawal of commuting subsidies is compensated by employers in the form of gross wage adjustments. We find zero gross wage effects for all workers, and small but significant increases for specific worker groups. Workers largely uncovered by collective wage agreements are compensated by 8% of their net wage losses. Nevertheless, the large share of net wage losses is carried by employees. We further look at how the policy reform's tax burden was distributed across worker groups and find that mostly high income workers suffer from the governmental commuting subsidy's withdrawal.

To motivate potential gross wage increases as a reaction to this policy change, consider a simple model in which firms and workers are matched in the labor market. The quality of the match (i.e. the worker's productivity) depends on the combination of the worker's skills and the specific tasks required for the job (see Brueckner et al. (2002) and Helsley and Strange (1990)). Workers bear the costs of commuting to their employers. They choose the firm that offers the highest gross wage net of travel expenses. The model makes interesting predictions about commuting reimbursement by firms. First, it explains why firms have an incentive to compensate workers for traveling. If certain workers match the firm's skill requirements perfectly but are mislocated (i.e. commuting

³Chapter 4 consists of Assmann et al. (2016).

distance to the firm is very large), the firm gains from compensating commuting as long as the firm's gains from the match with the mislocated worker exceed the expenses for this compensation. Second, the model predicts how the firms' commuting reimbursements react to a rise in transport costs, e.g. because of a reduction of governmental commuting subsidies. Again, if the firm's gains from the match are sufficiently large, the firm has an incentive to adjust gross wages in order to keep the productive but mislocated employees.

In our setting, we use a German policy reform, which in 2007 substantially reduced commuting subsidies for workers commuting more than 14 kilometers while leaving tax breaks for workers with commuting distances smaller than 14km unchanged. We use this variation in commuting costs to estimate whether and to what extent employers compensate their workers for travel expenses. A large and novel data set allows to estimate this effect precisely and consistently: We use a 25% sample of German record data which provides geo-referenced information on workers' exact place of work and place of residence. From these data and using GIS-software we construct a precise measure of road commuting distances, which has not been available so far. We find no wage adjustment for our full sample of workers. When analyzing the effect for subgroups, however, we find small but significant gross wage increases as a reaction to the policy change, which are robust to several specifications. Employees working in industries with a low coverage of collective wage agreements who are affected by the policy experience an increase in wages of 0.08 percentage points as opposed to those unaffected. This translates into a wage adjustment of 19 Euro after taxes, compared to an average net wage loss of 241 Euro per year.

In Germany, the sum of foregone tax revenues from tax breaks on commuting amounts to 4.5 billion Euro annually (Bundesministerium der Finanzen (2010)), which corresponds to 0.4% of overall public expenditures. These expenses are unlikely to be distributionally neutral. The obvious question is, whether they are progressive or regressive in nature, i.e., whether they benefit high or low-wage workers. We find that the regime prevailing 2007 is clearly regressive, while this effect is reduced substantially by introducing a lower bound on commuting distances for tax breaks. We further analyze regional differences and show that the reform has essentially equalized the distribution of tax savings across regions measured by population density. Comparing East and West Germany, the reform has reduced inequality in tax savings by about 30% between regions. These results are instructive beyond the German case, as we compare the two types of commuting subsidies most frequently applied (see Potter et al. (2006) for an overview).

In summary, my thesis provides the following research contributions. First, it shows that the correlation of migrants' individual wages and their concentration in residential neighborhoods is non-negative. Second, I provide credible evidence that there exist informal networks based on residential locations in the German labor market, which are

especially important for low qualified and minority workers. Third, I show that German workers are only compensated to a small extent in the absence of commuting subsidies. Additionally, designing commuting subsidies as a lump-sum deduction together with a lower bound on per-kilometer deduction leads to a more equal distribution of tax savings as opposed to without a lower bound.

Chapter 2

Residential Segregation of Immigrants in Germany

2.1 Introduction

On average, Germans earn 2,400 Euro per month before taxes, whereas people of foreign origin have a monthly income of only 1,800 Euro before taxes¹: A substantial gap. Part of this wage gap can be explained by the lack of transferability of human capital (see e.g. Aldashev et al. (2008)) and on migrants' lower human-capital endowments (Lang, 2005). Differences in the occupational choices made by foreigners (Constant and Massey, 2003) are also addressed in the literature. These reasons contribute to an inequality of opportunity: As shown in a broad literature on neighborhood and peer effects (see e.g. Case and Katz (1991), Glaeser et al. (2003), Marmaros and Sacerdote (2002) or Ludwig et al. (2008)), economic outcomes and opportunities depend not only on individual achievements but are also highly dependent on the behavior of individuals interacting in a non-work setting, e.g. the residential location and the mixture of groups residing in one's neighborhood. The central question in this paper is whether a degree of inequality can be ascribed to locational patterns. It is generally assumed that poorer neighborhoods mean worse "peer effects" and hence poorer economic opportunities. Potential channels of such effects are the lack of positive role models, because of common institutions and resources (Jencks and Mayer, 1990), such as worse public goods in worse neighborhoods, or because of a spatial mismatch (Kain, 1968). The latter means the spatial separation of job access and residential location of minorities. All these channels generate negative spillover effects for individuals living in worse city districts.

¹Numbers are from data of the German Employment Panel (BAP), which is used for the empirical analysis of this paper and presented in Section 2.3.

The data set used consists of individual data for a random 2% sample of German employees subject to social security from 1998 until 2007 on a quarterly basis. It contains labor market information, some socioeconomic information and residential information in form of city districts. As residential segregation is an urban phenomenon, five big German cities are selected into the sample: Berlin, Hamburg, Frankfurt/Main, Munich and Stuttgart.

The empirical strategy is twofold: First I use a broad panel data set for the years of 1998 until 2007 and estimate a fixed effects model to determine the effect of high concentration of foreigners on wages. There is a positive wage effect of the proportion of foreigners in one's neighborhood but a negative wage effect of being foreigner as opposed to German. Second, I apply a city level cross sectional estimation including also measures for educational exposure and segregation by income. I test whether the latter two effects lead to the main variation in wages as opposed to residential segregation. This can be rejected, as educational exposure and income segregation do not influence foreigners' earnings significantly. In summary, the empirical results tend to a positive network effect for foreigners, as they seem to profit from higher foreigner concentrations in their districts or cities respectively. The paper draws attention to a topic fairly neglected in research: How foreigner concentration and residential location affect individual earnings. Its empirical approach and the extensive data set used shed light onto foreigners' distribution among and within German cities as well as their income development over time.

The remainder of the paper is structured as follows: In Section 2.2, I summarize the theoretical and empirical background for the empirical approach. Section 2.3 gives an overview on the data and some descriptive statistics, Section 2.4 presents the empirical strategy and its results. Section 2.5 evaluates the results critically and concludes.

2.2 Literature Review

2.2.1 Measuring Segregation

Segregation is defined as unequal spatial distribution and concentration of a distinct group in a certain area. As Reardon and Sullivan (2004) emphasizes, segregation has two main dimensions: Firstly, it is a question of the groups' distribution inside a city. Are group members evenly distributed or do they cluster in one district? Secondly, what determines segregation is whether the group of interest is living side-by-side with other groups: Does the group isolate or exposure itself to non-group members? Duncan and Duncan (1955) establish measuring segregation with the Dissimilarity Index (DI). The Dissimilarity Index is defined as $DI = \frac{1}{2} \sum_{i=1}^N (\frac{group_i}{group_{total}} - \frac{nongroup_i}{nongroup_{total}})$, where group represents a minority group and nongroup the reference group (here migrants will be

group and nongroup Germans respectively). i indexes for the neighborhood and $total$ are all neighborhoods in a city. The DI ranges from 0 to 1 and focuses on the group's evenness of distribution throughout the smaller geographical areas which form a city. It can be interpreted as the proportion of the minority group that would have to change the residential area in order to generate an even distribution throughout the city.

The second established measure of segregation is the Isolation Index (II), which measures the isolation of a group in the sense of living only with group members. The Isolation Index is defined as $II = \frac{\sum_{i=1}^n [\frac{group_i}{group_{total}} * \frac{group_i}{population_i}] - \frac{group_{total}}{population_{total}}}{\min(1, \frac{group_{total}}{population_{total}}) - \frac{group_{total}}{population_{total}}}$. It describes the extent to which the neighborhood-level group share experienced by the average group member exceeds the level that would be expected under perfect integration. High values mean a high level of isolation of the minority group, whereas a value near 0 implies a high degree of mixing with other groups. These indices are used as measures of unequal distribution in the empirical analysis in Section 2.4.

2.2.2 Consequences of Residential Segregation

Consequences of segregation can be positive as well as negative. Which effects dominate for whom and how they can be separated is discussed in “Are ghettos good or bad?” by Cutler and Glaeser (1997). The authors suggest that racial segregation can proxy for segregation by skill which is what actually harms blacks, the assumed lower skilled minority group. They show that increased racial discrimination can lead either to a positive or negative utility effect for blacks: Either it means fewer black skilled will live in black areas which leads to a decrease in utility². If in contrast enhanced racial segregation is not equivalent to segregation by skill, blacks will profit from an increase in racial discrimination³. Consequently, a reduction in discrimination and segregation does not necessarily lead to greater equality among groups. The authors also present an empirical approach and discuss some difficulties: When public goods can be selected by choosing residential location, more skilled and therefore wealthier blacks will move to richer (white) areas. Consequently, in terms of intracity segregation the effect on economic outcomes will be overstated. As a result of their theoretical model the authors measure the impact of segregation on a city level and compare outcomes between more and less segregated cities.

Cutler and Glaeser (1997) use the dissimilarity index to measure segregation on economic and educational outcomes. The focus of interpretation in their work lies on the effect

²As the neighborhood's average human capital influences individuals' utility, skilled blacks moving out of predominantly black neighborhoods leads to a reduction in utility for the remaining black inhabitants. Especially skilled blacks will benefit from a higher share of skilled blacks in their own neighborhood.

³Skilled blacks are facing a market failure because they cannot internalize the positive externality of skilled blacks moving into black neighborhoods. Hence, the discrimination costs serve as a tax helping to internalize costs imposed on their own community by moving. (Cutler and Glaeser, 1997)

of segregation on blacks when additionally controlling for an index for black income segregation and educational exposure. The income segregation index⁴ is generated on a city level. Its correlation with racial segregation is 0.7 which gives rise to the assumption that the one might proxy for the other. Data from the Census Public Use Micro Sample from 1990 is used and only young people are included in the sample to evade the problem of endogenous residential location, in this case between cities. Only males between age 18 and 25 are included in the sample. The authors argue that young individuals do not chose their residential location themselves but their locational choice is exogenously given. This assumption is quite strong, especially regarding the decision makers in this scenario: Individuals' parents. The authors show that segregation significantly hurts blacks comparatively more than whites.⁵

To test the interdependence of segregation by skill and by race, they first test whether blacks are harmed more by racial segregation in cities with higher income segregation. On that account a measure for income segregation is included in the regression. Second, to test whether the worse performance of blacks is a result of low interaction with positive role models - represented by the higher qualified white population - an exposure to high education measure is included.⁶ The results show that segregation has a significantly negative effect on blacks' incomes. The authors do not find support for an influence of income segregation in their data, as effects on the economic outcome variables are not statistically significant. Educational spillover, however, influences blacks' income positively.

Cutler et al. (2008) investigate the effect of segregation on educational and economic outcomes of particularly young immigrants to the United States. The sample used consists of census data only for young male immigrants (age 20-30 years). The effect of segregation here is given by the group share of the tract population, which is measured via fixed effects estimations. For higher group shares, earnings are significantly smaller than for lower ones, which reflects a negative impact of segregation on economic outcome. As the coefficient for group share on MSA level has a positive sign, the authors conclude that only an increase in tract level concentration not related to the overall MSA share leads to negative economic outcomes. Apart from that, Cutler et al. (2008) find that

⁴The Black Income Segregation index is defined as $IS = \frac{1}{2} \sum_{i=1}^N \left| \frac{RichBlack,i}{RichBlack,total} - \frac{NonrichBlack,i}{NonrichBlack,total} \right|$, where rich blacks are those earning more than the 0.75 percentile of the black income distribution and non-rich are all others.

⁵To bypass endogeneity, several instruments are included to substitute residential segregation. One comprehends public finance, arguing in a Tiebout model environment: With a greater variety of local governments and taxes within an area, individuals have a greater incentive to "vote with their feet" and relocate given their preferences about public goods. Thus, sorting may be explicable in this way.

⁶The index for educational exposure is defined as $EE = \sum_{i=1}^N \left(\frac{Black_i}{Black} \right) * \left(\frac{Educ_i}{Persons_i} \right) - \left(\frac{Educ}{Persons} \right)$, which represents the percentage of the average black's census tract that is educated, where educated people are defined as having attended college. The Educational Exposure index (EE) lies around zero and takes on positive values when blacks are exposed to higher education in their residential area and is negative if the education in their neighborhood is low.

American cities' segregation is mostly driven by segregation by income as opposed to segregation by origin. In Section 2.4, I will build upon these two articles to test the interplay of segregation and wages for German cities.

2.3 Data

In this paper, I use the slightly anonymized employment panel (BAP), which is provided by the Institute of Employment Research (IAB). The BAP is a panel of micro data from 1998 to 2007 on a quarterly basis. It is registry data from the German Federal Employment Agency which collects labor market relevant data for all employees subject to social security. This means self-employed and marginally employed individuals are not part of the sample. A disadvantage of the data set is the reference only to individual data, not to households⁷. As segregation is an urban phenomenon, five big German cities are selected: Berlin, Hamburg, Frankfurt/Main, Stuttgart and Munich. The first three are Germany's biggest cities. Stuttgart is part of the sample because of its history of "Gastarbeiter" and because it has the largest percentage of foreigners in the sample: 20.54% overall. To examine the impact of residential segregation on individual income, it is important to consider smaller observation units in each city. Ideally neighborhoods should be considered for the research's purpose. In this case, the smallest units available are offices of the employment agency, which correspond to districts within cities. The sample consists of 44 geographical units of varying size⁸.

The remaining question is how to define "foreigners". What determines a foreigner or a German in this data set is one's nationality; this of course ignores all foreign borns with German nationality and "second generation" migrants, who are not identified in the given data set. As Aldashev et al. (2008) point out, over 50% of all foreigners living in Germany in 2005 also had German nationality⁹. Furthermore, distinguishing between "foreign" and "German nationality" does not really fit the problem of earnings inequality, as figure 2.1 shows. The two groups considered are called foreigner type 1 and type 2 (F1 and F2)¹⁰. To differentiate between the two groups, per capita income of all countries of origin available in the sample is compared (the definition follows the OECD

⁷Also information about children or an additional earner in the household is missing. This makes it difficult to analyze households' decision problems. I therefore approximate households with individual data.

⁸An overview on all included residential districts is given in appendix A.

⁹Furthermore, the data set contains no information on duration of stay in Germany, which is why it is not possible to account for assimilation and therefore equalization effects of longer stays in Germany, as it is done in Lang (2005).

¹⁰It would be desirable to differentiate foreigner types further e.g. into older and newer migration groups, as one could expect the former ones to have built networks within Germany. But as descriptives show, the number of F2 foreigners is very small in some cities anyway such that finer classification would probably lead to a loss of explanatory power.

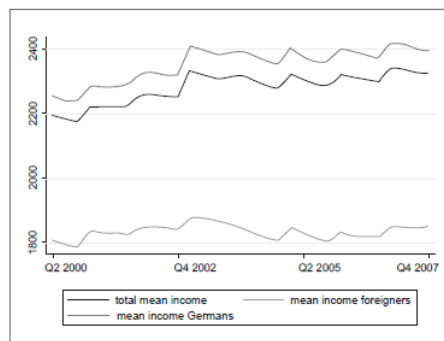


FIGURE 2.1: Mean Earnings for Germans and Types of Foreigners for 1998-2007 on quarterly basis

data base from 2011). Foreigners whose home countries have a per capita income greater than that of Germany will be assigned foreigner type 1, the rest type 2. The underlying assumption is that individuals having a higher income in their home country are only likely to move to another country if their income is even higher there to compensate costs of migration. Type 1 foreigners mainly consist of western and northern European countries plus USA, Canada and Australia. F2 foreigners embrace the home countries of traditional German guest workers (southern Europe, Turkey and Tunisia) and “new” immigrant countries like Eastern European and Asian countries.¹¹ As figure 2.1 shows, foreigners of type 1 seem even more affluent than Germans. Moreover, their mean income follows a slight upward trend over the time regarded in the sample. In contrast, F2 have a constantly lower mean income. F1 probably does not generate negative spillovers for Germans. Consequently, I use only F2 for the analysis of segregation and income differentials.

2.3.1 Descriptive Statistics

Table 2.1 shows descriptive statistics¹² of the selected cities. As described in Section 2.2, the indices point to different aspects of segregation and its impacts on minority groups, F2 in this case. Dissimilarity measures the evenness of a group’s distribution in a city. In Germany, Berlin has the highest score: 30% of F2 would have to move to generate an even distribution across districts. Glitz (2014) looks at workplace and residential segregation of immigrants in Western Germany. He finds values of 0.14 in 2000 and 0.15

¹¹Overall the cross sectional data consists of 3,935 F2 foreigners. 70.4% and hence the great majority of these individuals come from the former guest worker countries. An overview on numbers of migrants and origin is reported in appendix A.

¹²Note that the numbers presented in table 2.1 rely crucially on the definition of F2 foreigners: We can account for a sample of employees subject to social security with the defined nationality. That is, interpreting the percentages of F2 in the different cities ignores second generation migrants and refugees, as all unemployed, all persons marginally employed and benefit recipients. This may lead to a different picture than expected.

TABLE 2.1: Descriptive Statistics on Migrant Segregation, 2007

City	N	Percentage F2	DI	II	Mean earnings*
Hamburg	8,326	6.34	0.2167	0.0768	2,580
Frankfurt/ M.	7,206	10.30	0.1704	0.1196	2,872
Stuttgart	5,862	12.85	0.1644	0.1468	2,744
Munich	13,065	10.41	0.1620	0.1193	2,859
Berlin	10,041	3.96	0.3019	0.0586	2,353

All numbers from BAP June 2007. Average wage is average monthly working income in the sample. F2 denotes foreigners in whose country of origin per capita income is smaller than that of Germany. * EUR per month (before taxes).

in 2005 for the whole of Germany, which is comparable in magnitude.

The Isolation Index describes the extent to which the minority group lives isolated and only with group members. Berlin has the lowest level, whereas Stuttgart reaches the highest although still quite moderate level.

Frankfurt is well known for problems with immigrant integration, which is not reflected in the segregation indices. Munich exhibits low levels of segregation on both scales. Although the overall share of F2 foreigners is comparatively high, mean income in Munich is high and the resident foreigners seem to be spread well over the city. Another question that might arise from this descriptive analysis is, whether distinct dimensions of segregation may influence the economic outcome in a different way.

Some caution should be applied when interpreting these indices. The classification of residential areas varies widely across the selected sample: Berlin has the finest categorization and counts 12 administrative units within the municipality, Hamburg and Stuttgart are divided in seven units and Munich and Frankfurt only in five. This is a clear limitation of the data set which may affect the results of the study, however this problem cannot be surmounted at this point.

A perhaps surprising feature is the relatively low proportion of F2 foreigners, especially in Berlin. The given sample consists only workers in jobs subject to social security. Furthermore, the restrictive definition of foreigners as individuals of foreign nationality and the selection of F2 countries should be kept in mind. Although the percentage of foreigners varies across cities, the mean earnings are quite similar. In view of this, the hypothesis that pure numbers of foreigners (absolute or relative) affect mean incomes negatively can not be supported. It is therefore useful to consider levels of segregation in the cities.

It is evident that the phenomenon of segregation is less severe than it is for the United States. In American cities, the DI ranges from 0.35 to a maximum of 0.62 (Cutler et al., 2008). This means even the least segregated American city exhibits a higher level of segregation than Berlin, the city with the highest level in my sample. This first result should be kept in mind for all subsequent interpretations.

TABLE 2.2: Descriptive Statistics Germans vs. Immigrants, June 2007

Variable	Germans	F2 Migrant
Mean monthly wage	2,702 (1533)	2,094 (1329)
Mean education	13.18 (2.83)	11.5 (2.77)
Share F2 in district	0.09	0.13
Proportion working part time	0.22	0.25
Proportion female	0.50	0.46

Standard deviations are listed in parentheses. F2 denotes foreigners in whose country of origin per capita income is smaller than that of Germany.

Apart from the differences across cities, I am interested in differences between the minority and majority group of my analysis. Table 2.2 summarizes some variables of main interest for Germans and F2 from 2007. F2 earn on average less per month and are on average also less educated. Their average years of education are only 11.5 compared to 13.2 for Germans. Not surprisingly, the proportion of foreigners living in individuals' residential area is about one third higher for F2 than for Germans.

Overall, the descriptive statistics suggest variation first in geographical entities and second between Germans and non-Germans. Whether these differences can be attributed to income differences is discussed in the next section.

2.4 Empirical Analysis

2.4.1 Fixed Effects Estimation

The first empirical strategy is the approach suggested by Cutler et al. (2008) using the panel data set described in Section 2.3. The general fixed effects panel estimation is described in equation (2.4.1).

$$y_{i,t} = \alpha_i + x'_{i,t}\beta + \varepsilon_{i,t} \quad (2.1)$$

with i indexing individuals, t being the time period, ε being an i.i.d. disturbance. α_i is a vector of unobserved, individual but time invariant effects which captures unobserved heterogeneity. The dependent variable in the Fixed Effects model is the logarithm of monthly earnings. I use a fixed effects model to eliminate unobserved time invariant effects on individual wage. The variation to identify differences in earnings as a result of residential location will thereby only be a result of changes in foreigner shares or concentration within a district. The variables of main interest are a dummy for being a foreigner of type 2 (F2), the proportion of F2 living together in one residential district (F2district) as well as an interaction term (F2×F2district), which measures the impact

TABLE 2.3: Fixed Effects Panel Estimation

Variable	Ind. FE
F2district	.0885** (.0441)
F2×F2district	.0021 (.1132)
F2 × agecity	.0530 (.0519)
F2 × educcity	.1973 (.1318)
DI	-.5671* (.3350)
II	.0713 (.6439)
Adj. R^2	.3142
N	1.48 Mio
Cluster and heteroskedasticity robust standard errors listed in parentheses. */**/** mark significance at the 10%/5%/1% significance level. F2 denotes migrants whose country of origin has a smaller per capita income than Germany (see Section 2.2). <i>agecity</i> denotes the mean age of migrants in a city and proxies for assimilation effects. <i>educcity</i> is F2's mean education in a city.	

of district concentration for F2. Additionally, a set of control variables is included to account for differences in education and occupation, which are also main drivers for immigrant wage gaps (see e.g. Lang (2005), Constant and Massey (2003) and Aldashev et al. (2008))¹³.

The estimation addresses the question of central interest: Does living in neighborhoods with a high proportion of migrants have an impact on monthly income? And does this impact differ for Germans living in such districts as opposed to foreigners themselves?

The impact of proportion of foreigners in the neighborhood on income is positive and significant. As in Cutler et al. (2008), variables indicating city wide information on foreigners are also included in the estimation. *F2agecity* is a city specific variable which determines the mean age of F2 individuals in a city. It is used as a proxy for duration of the immigrants' stay, since they are assumed to be more assimilated the longer they stay in a country. This duration of stay has no significant impact on earnings, as well as *F2educcity* (a city wide group mean, which states the mean level of immigrants' education). The effect of uneven distribution of foreigners (DI) in a city stays negative but only significant at a 10% level. The Isolation Index's effect is not significantly different from 0, which is in line with Cutler et al. (2008). This suggests that isolation has no impact on individual earnings. Regarding the goodness-of-fit, the model seems to be

¹³Control variables and definitions are listed in appendix A.

quite good specified as they explain 31% of the sample variation.

All in all, the picture drawn by the panel regression is diverse: On the one hand, concentration in a neighborhood seems to have a positive effect on wages, as reflected by the positive sign of *F2district*. A possible explanation is a network effect where potentially “more similar” individuals help each other to find a job. On the other hand concentration within a city negatively affects income of both foreigners and Germans. One driver of the effect could be that, overall, a higher concentration of a group with lower socioeconomic background characteristics (see table A.1) diminishes overall earnings, because of negative role models or worsening public goods¹⁴.

Apart from those effects, all other variables of interest are not statistically different from zero. A major problem in this fixed effects specification is the small variation in the variables of interest over time.

I conduct several robustness checks, including different specifications¹⁵. I test for random effects (which could solve the problem of low time variation) and the possibility of pooled OLS using a Hausman test, which both have to be rejected. To control for sample selection, I restrict the sample to males only, as they have a higher workforce participation. The results again stay qualitatively the same. I conclude that insufficient time variation is the main problem in this specification, which is why I exploit another type of variation in the next section.

2.4.2 Intercity Approach

Following Cutler and Glaeser (1997), mobility is assumed to occur only on a city level¹⁶. As opposed to the last section I use cross sectional data from June 2007, containing 48,476 observations. Additional to the controls used in 2.4.1, the measures for income segregation and educational exposure are included in the regression. I do so to test whether these proxy for residential segregation’s effect on earnings¹⁷. To get a first impression, their city wide levels are listed in table 2.4. As table 2.4 shows, there is only little segregation by income in most of the cities, only Berlin constitutes an exception. Munich has the lowest level, where only 2.87% of the rich foreigners would have to move

¹⁴E.g. children with worse socioeconomic background can affect children in their classes negatively. Carrell and Hoekstra (2010) show how children subject to domestic violence have a negative causal effect on their classmates’ performance and behavior.

¹⁵Instead of individual FE I also estimate the model using country of origin and city FE. None of these specifications change the results qualitatively, and the impact of the variables of main interest again is mostly insignificant.

¹⁶Considering the cities in the sample, which are geographically far spread in Germany, the assumption seems reasonable.

¹⁷To compute the income segregation index, foreigners are divided into rich and non-rich. The income segregation index (IS) depicts how many rich foreigners would have to move to generate an even income distribution across the city. As in Cutler and Glaeser (1997), foreigners are defined to be rich if their monthly income lies above the 0.75 percentile of foreigner F2’s income distribution (the 0.75 percentile is 2,958 Euro per month). Non-rich are all individuals earning less or equal.

TABLE 2.4: Income Segregation and Educational Exposure

City	Income Segregation	Educational Exposure
Hamburg	0.1078	0.7469
Frankfurt/ M.	0.0370	0.7323
Stuttgart	0.0595	0.6393
Munich	0.0287	0.7346
Berlin	0.2554	0.8383

Note: The data is based on cross sectional data from June 2007 and contains 48,476 observations.

to generate an even income distribution within the city. A reason for that might be the overall high income level in Munich and the small number of geographical units there. For the educational exposure index (EE)¹⁸ high levels implicate a high degree of integration. Here Berlin is most integrated whereas Stuttgart has the lowest level of educational exposure. Here the problem of sorting by education seems most severe, although again values for German cities are comparably low.

I include no residential area variables, but only city wide segregation measures plus city control variables, to ensure that the estimates of main interest are not driven by differences between cities¹⁹. To estimate segregation on the city level, I follow the specification proposed by Cutler and Glaeser (1997):

$$\begin{aligned} \ln(wage)_i = & \alpha + \beta_1 \ln(population)_c + \beta_2 \ln(income)_c + \beta_3 DI_c + \beta_4 DI_c \times F2_i + \beta_5 II_c + \\ & \beta_6 II_c \times F2_i + \beta_7 IS_c + \beta_8 IS_c \times F2_i + \beta_9 ES_c + \beta_{10} ES_c \times F2_i + X_i' \gamma + \epsilon_i \end{aligned} \quad (2.2)$$

According to the interpretation of Cutler and Glaeser (1997), the focus in the following lies on the interaction term of the segregation indices and a dummy for being F2 foreigner. Consider the first column: The city controls are significant as is the measure for segregation. Hence, the before measured effects are not purely city wide effects accounting for wage levels, but do reflect the actual effect of residential segregation. Uneven distribution within a city has a significant negative effect on foreigners' own earnings ($DI \times F2$). The effect for Germans (DI) in contrast is positive and insignificant. Isolation, however, is positive and statistically significant both for Germans and foreigners. Again, this suggests the interpretation of beneficial social group networks. The effect for Germans is greater in magnitude than that for foreigners, which may reflect a majority effect.

Column (2) includes tests for two further hypotheses: First, I test whether residential

¹⁸For computing educational segregation index all individuals are taken into account to measure foreigners' exposure to highly educated individuals. Everyone having at least "German Abitur" is considered as highly educated.

¹⁹Such differences could consist of general differences in the local labor markets as job opportunities, general wage levels or other differences in structures or institutions.

TABLE 2.5: Intercity Cross Sectional Estimation

	W/o additional indices (1)	Incl. IS and EE (2)
$\ln(\text{population})_c$.0317** (.0113)	.0394** (.0135)
$\ln(\text{income})_c$.2481** (.0990)	.5419*** (.1207)
DI	.3285 (.3690)	
DI \times F2	-.02177** (.1017)	-2.6751 (2.7342)
II	.9697*** (.2998)	
II \times F2	.3566** (.1671)	.7607* (.3905)
IS		.4445* (.2375)
IS \times F2		1.5017 (1.4867)
EE		-.3499*** (.1113)
EE \times F2		.4182 (.5405)
Adj. R^2	.6091	.6091
N	48,476	48,476

Cluster and heteroskedasticity robust standard errors listed in parentheses. */**/
*** mark significance at the 10%/5%/1% significance level. F2 denotes foreigners, whose
country of origin has a smaller per capita income than Germany, see Section 2.3. DI
and II are segregation indices, IS is an index for integration by skill. EE denotes
educational exposure.

segregation by origin is just a proxy for segregation by income, which is done by including the IS. Second, I test whether foreigners earn less because they live away from positive role models by including the educational exposure index, measuring the degree to which foreigners are in contact with highly educated individuals, based on their residential location.

$DI \times F2$ ²⁰ stays negative but is insignificant. This means with income and educational segregation measures as a control, uneven distribution by origin has no effect significantly different from zero on foreigners' wages. Again the coefficient on II has a positive sign and is slightly significant. Isolation hence is still beneficial for migrants but the reliability of its effect is dubious. Income segregation (IS) has a positive sign with significance at 10% level: This would mean income segregation is beneficial for Germans. When Germans benefit from income segregation, for their group spillover effects are more important. This result is consistent with Cutler and Glaeser (1997), where skilled

²⁰The effects of housing segregation on Germans are omitted because of perfect collinearity in this specification.

groups always profit more from spillovers. The interaction with F2 is also positive, but insignificant.

The educational exposure index (EE) is highly significant for Germans and has a negative sign. Educational integration of foreigners hence influences German wages negatively. This result is surprising at first glance. A possible interpretation might be that Germans profit from having exclusive access to education and thus better job access than foreign workers. But there may also be other forces driving the variation, and the result by itself does not seem convincing.

In column (2) neither residential segregation nor segregation by income provide reliable results. The hypothesis of income segregation proxying for a sorting by origin hence cannot be supported by the data: Income segregation is significant only for Germans. The second hypothesis can be rejected: For migrants there is no significant effect on earnings when being exposed to highly educated individuals. A reason for that result may be the little variation in the indices as presented in table 4 as well as the too broad classification of “neighborhoods” which again determines the potential interaction.

Regarding the goodness of fit measures, both specifications have the same adjusted R^2 . The levels are quite high, but as the magnitude does not change when adding the IS and EE, these measures do not seem to have any additional explanatory power. It appears their inclusion is not an appropriate explanation to the German-foreigner wage gap.

Comparing these results to those presented by Cutler and Glaeser (1997), who investigate effects on economic outcomes for immigrants to the US, some differences are apparent. First, the authors’ hypotheses cannot be confirmed for German cities. Second, segregation seems to have an arbitrary effect in the German case. The authors of the role paper include only a dissimilarity index whereas I include an isolation index as well.²¹ As well as in Cutler and Glaeser (1997), in the German case uneven distribution (DI) seems to have a negative effect on foreigners’ earnings.

2.5 Conclusion

Some caution about causal interpretation is appropriate in this setting: First of all, there is an endogeneity problem with respect to self sorting into districts within a city. One way Cutler et al. (2008) and Cutler and Glaeser (1997) try to handle this issue is to only include men younger than 25 in the sample, because they are assumed not to decide where they live on their own. They do not only investigate segregation’s impact on earnings, but also on other economic outcomes. In this paper, using only young individuals is a too strong restriction. First the data set only contains working population

²¹This is done mainly because of consistency in procedure but also to emphasize the different aspects of segregation’s appearance.

and does not include information on youths in their education. Better qualified individuals may be excluded from the outset. Especially better qualified foreigners might hence be excluded from the sample. Individuals achieving a university degree are part of the workforce only from an age of 25 years onwards, for foreigners probably even later, if they e.g. lose a year in school because they have to learn the German language first. This procedure would lead to selection bias, and thus this approach seems not to be suitable for the purpose of this work. This may lead to a bias on the estimates and the standard errors. For this reason, I only interpret the direction of the effect as well as the relative magnitude as compared to the other estimates. However, if we suppose that especially unskilled immigrants move to district with a community of their own nationality to overcome linguistic and cultural barriers (negative self selection), then this would be associated with a negative income effect. If I still find a positive effect for foreigners in Germany, this might point to an even higher value of networks.

Apart from that, there is another severe problem considering the data: To observe effects from living in a specific area, the definition of this district is crucial. As a result of data restrictions and privacy regulations, the “neighborhood” definition applied in this paper is quite broad (as districts encompass very different areas like “Berlin Mitte” and “Munich City”) and can give only a hint on the importance of inner city inequalities. Another severe problem with the data set used is the definition of migrants by their nationality, which leaves out a great proportion of migrants with German nationality and can thereby bias estimates on wage differences (see Aldashev et al. (2008)). However, these drawbacks make it even more important to continue research in this important field especially for a country like Germany, where the debate on immigrant integration has become a constant part of political debate. This quantitatively based research could also be augmented by a qualitative analysis which could improve the understanding of the complicated issues of segregation and inequality. The first important result of this work is that the magnitude of segregation in German cities is much less severe than residential segregation is e.g. in the US. Second, uneven distribution of foreigners has a negative effect on earnings in both estimated models. Living isolated from other groups, foreigners living in German cities tend to profit from an enhanced contact to their own kind. Networks to find a job and overcome cultural and linguistic barriers may play a role in this finding. Although integration is politically aimed, it might not always be a beneficial concept for all groups. Immigrants are on average a group with lower socioeconomic background characteristics. A high concentration of lower socioeconomic characteristics might affect public goods. Another important issue in this context is how these effects might affect housing values and real estate prices in such neighborhoods and cities overall. Future research should test whether effects on public goods translate negatively into house price changes in those areas. For example, Kane et al. (2005) find that house prices react to a change of sociodemographic compositions of schools, the

public goods influencing locational choice and therefore neighborhood housing prices the most. Both income effect and the indirect effect on public goods can affect housing prices negatively.

Combining with the positive network effect discerned in this paper, integration policy needs to be carefully considered: Desegregation policies might help to diminish the negative effect on public goods on housing prices and overall welfare. But these policies stand in contrast with positive network effect for immigrants. More careful research should be done to identify the causality of effects. However, policies affecting public goods in districts highly populated by immigrants appear to be a more appropriate instrument than general residential policies.

Chapter 3

Job referrals based on residential location networks

3.1 Introduction

In this chapter, I want to overcome the main problems of chapter 2, namely the inaccuracy of the neighborhood definition and the improper control for self-selection. I therefore supply more precise and valid evidence for the interplay of space and labor markets. I analyze the existence of informal networks based on residential locations. These networks can be used to find a job and therefore link the labor market directly to peoples' location in space.

This chapter is based on joint work with Norbert Schanne (IAB Nuremberg) and consists of a subsequent version of Hawranek and Schanne (2014) and Hawranek and Schanne (2015).

We examine how residential neighborhoods can serve as a pool of information for an informal labor market and investigate the effect of job referrals through one's residential location. In particular, we analyze the relationship between living and working together in the context of job referrals in the Rhine-Ruhr metropolitan area. The Rhine-Ruhr is Germany's largest and the EU's second largest agglomeration, located in North Rhine-Westphalia. It is spread across 7,110 km^2 including big cities like Cologne, Düsseldorf and Dortmund. The metropolitan area is home to over 11 million inhabitants and is especially interesting for urban analysis due to its densely populated nature and the economic diversity.¹

¹Traditionally, the Rhine-Ruhr was specialized in heavy industry and mining. The structural change in the 1960s lead to a specialization in the service sector. Until today, the area is economically contrasting with high unemployment rates in Dortmund and Gelsenkirchen on the one hand and the prospering Rhine area on the other hand. See figure B.1 in Appendix B.

We use a novel data set covering geo-coded record data for the entire working population (liable to social security) and all corresponding establishments. As social interaction is not measurable directly with any kind of administrative data, we use a well-established approach to approximate local network effects following Bayer et al. (2008): We estimate the propensity of two individuals to work at the same place when residing in the same neighborhood (reported with an accuracy of 500m×500m grid cells) with a linear probability model (LPM). We compare this effect to the propensity of two individuals residing in adjacent neighborhoods, conditional on a super-neighborhood fixed effect (where super-neighborhoods are all adjacent neighborhood grid cells).

We rule out several alternative explanations for this propensity effect, in particular a reverse direction of housing referrals amongst colleagues and the spurious correlation due to the geography of workplaces and transportation infrastructure, by conducting a number of robustness checks. This makes us confident that we interpret the measured effect as an indication for job referrals where information on an informal job market circulating in one's residential neighborhood.

Our approximation of the referral effect does not allow us to differentiate who (within a pair) benefits from this local network effect. In this chapter we focus on identifying the existence and credibility of a residential referral effect. Our network effect clearly is an approximation for network activity. Our estimates can only be a lower bound as presumably other forms of networks exist in an informal job market. The literature distinguishes three types of informal job market networks: Networks of former coworkers or classmates (see e.g. Glitz (2015), Dustmann et al. (2014), Kramarz and Thesmar (2013), Marmaros and Sacerdote (2002), Hensvik and Nordström Skans (forthcoming) and Saygin et al. (2014)), family networks (see e.g. Kramarz and Thesmar (2013)), and residence based networks (see e.g. Bayer et al. (2008), Schmutte (2015), Hellerstein et al. (2011)). We show that residence based networks play an important role, especially for low skilled workers and ethnic minorities.

In this chapter, we look at how referral effects based on residential location may differ for a European country as opposed to US American data, given that institutional backgrounds and cultural conventions are quite different with respect to the labor market and job search. In addition, we are able to investigate a number of issues in order to shed further light on actual job referral effects: First, our data allows us to distinguish between the effects on working in the same neighborhood and working in the same establishment - probably the more accurate measure for job referrals. Second, an advantage of the data set we use is the overlapping structure of our reference groups, the so-called super-neighborhoods. A crucial assumption for the identification of social interaction is that there is no sorting by unobservables within these reference groups. When conditioning on a fixed reference group as in Bayer et al. (2008) or Schmutte (2015), this assumption is less likely to hold as when using a rolling window design.

Third, we analyze to what extent the findings are due to highly concentrated clusters of employment opportunities in central business districts, and find small positive bias in the referral to a neighborhood. Finally, we address to what extent people tend to work in their residential neighborhood, and whether the evidence in the literature is affected by inadequately accounting for short-distance commuting behavior. In contrast to previous work, we also incorporate data on commuting networks and find them not to be the driver of our measured interaction effect.

The remainder of the chapter is structured as follows. Section 3.2 gives an overview on the related literature. Section 3.3 describes the data set we use for the German Rhine-Ruhr area. Section 3.4 presents the research design and the baseline model. In Section 3.5 we discuss our results and robustness checks and further specifications in Section 3.6. Section 3.7 concludes.

3.2 Literature Review on Neighborhood and Network Effects

The connection between labor market outcomes and residential neighborhoods is twofold: First, there is a strand of literature regarding the direct effect of neighborhoods on individual behavior which is then translated into economic outcomes. Second, residential locations can serve as a meeting point and hence be interpreted to create social networks².

If residential location plays the role of a social peer group, which influences individual behavior via peer pressure (for unemployed to find a job) or through the need to resemble your peers (neighbors), then one can expect that people in a neighborhood with a high share of employed neighbors will be likely to find a new job when unemployed more quickly. This is also the case, when neighborhoods serve as a pool of information to build networks where information on new job opportunities flows. In this latter case, one can expect individuals to find a job at their neighbors' firms more quickly, if the share of employed neighbors is higher.

In this second chapter, I focus on social interactions in the form of network effects on an informal job market. As Montgomery (1991) points out, the labor market is characterized by asymmetric information. Employers cannot observe applicants' true productivity, such that they have an incentive to rely on referrals from their own employees to reduce search costs and avoid adverse selection.³ Additionally, Topa (2001)

²An overview on neighborhood and network effects in a very general setting can be found in Topa and Zenou (2014).

³Hensvik and Nordström Skans (forthcoming) test the model empirically and show that Swedish employers use employees' networks as a signal for their productivity.

shows that workers have an incentive to share information on available jobs with their network while being employed, as this information sharing serves as an insurance in the event of unemployment. Hence, in equilibrium one should observe assortative matching of employees with their social networks. Conley and Topa (1999) suggest, residential neighborhoods are a natural form of networks for information exchange. Neighborhoods qualify as information transmission environments, because of the low transportation costs within neighborhoods (both monetary and time) and because of local institutions, such as schools, churches or clubs, where people can meet and interact.

The work most related to ours is Bayer et al. (2008), who also estimate the propensity of working together, when living in the same as opposed to a nearby neighborhood. They use the 1990 U.S. Census of Population for the Boston metropolitan area and define census blocks as neighborhoods and census block groups as super-neighborhoods. We choose this paper as a point of departure, as the authors make a strong case for identifying social interaction in a very specific way, given the assumption of no correlation in unobservables within super-neighborhoods. In contrast to Bayer et al. (2008), our data provides information on the exact establishments of workers. This specification reflects a referral effect much more realistically. First, because theory (e.g. Montgomery (1991)) suggests that employers have an incentive to hire workers' social contacts. Therefore we expect to observe workers from the same network at a firm level. Second, it is more likely that workers gather information on job openings in their own establishment rather than its neighborhood.

Numerous other papers emphasize the importance of informal job markets like Ioannides and Loury (2004) and Corcoran et al. (1980) using US data. Brenzel et al. (2016) show that about every third position is filled with a friend or acquaintance of the staff in Germany. Glitz (2014) and Dustmann et al. (2014) investigate the effects of coworker networks on labor market outcomes using German and Saygin et al. (2014) using Austrian record data. Glitz (2014) finds strong positive effects on own probability of working and wages, which indicates significant effects of social networks in the German labor force on labor market outcomes.

Ioannides and Loury (2004) summarize stylized facts on the usage of informal job search channels. About 15% of unemployed Americans use friends and acquaintances for job search.⁴ They report variation in the usage of such information channels among age and socioeconomic groups: E.g. women and higher educated people use friends and family less often whereas the findings for older people are opposing.⁵ Kramarz and Thesmar

⁴Using the PSID 1993, Ioannides and Loury (2004) find that 15.5% of unemployed and 8.5% of employed ask friends and relatives about potential job openings.

⁵Portis (1993) find increased usage of informal channels for 45-55 year-olds and 55-65 year-olds in 1992 respectively analyzing CPS data. On the contrary, e.g. Corcoran et al. (1980) report that usage of informal job market declines with age and/or work experience. Holzer (1987) finds that especially young people aged 16 to 23 rely on friends and relatives in 60-70% of all jobs they actually attain (using data on search methods from the 1981 NLSY).

(2013) analyze how networks of families affect labor market outcomes. Following Granovetter (1973), they distinguish between how strong ties (namely family) and weak ties (like classmates and neighbors) affect the decisions of Swedish youths entering the labor market. They use a population wide data set linking graduation records and family ties to longitudinal matched employer-employee data and find that the effect of strong ties is important, but only significant if one parent is currently employed at the same plant. The effect is stronger for low educated youths and for immigrants.

Pellizzari (2010) shows⁶ that about 30% of Germans use personal contacts for finding a job whereas only about 15% of US Americans use such search channels. These numbers suggests that job referrals play an even more important role in European countries as compared to the US⁷.

Hellerstein et al. (2011) test for the presence and importance of residential networks (defined by census tracts) to determine the assignment of workers to establishments. They use a measure of workplace segregation by residential origin and compare it to the share of coworkers that would come from the same neighborhood if workplaces were randomly assigned. They find a significant role of residence based networks using US 2000 Decennial Employer-Employee Database (DEED), which is especially strong for unskilled workers, minorities and workers in small establishments. Using a similar method and the same data, Hellerstein et al. (2014) investigate the outcome of residence-based labor market networks. They find a positive earnings effects and a lower turnover for workers more connected to their neighbors, especially if neighbors have the same race and hence interpret their results as evidence for productivity enhancing spillovers. Schmutte (2015) also studies the effects of residence based networks on earnings. Identifying local interaction with the design of Bayer et al. (2008) as a first stage, he uses LEHD US employer-employee data to estimate an employer-specific wage premium. He finds that workers living in a neighborhood with high-quality networks are more likely to move to a better paying job. In particular, a one standard deviation increase in network quality is associated with a 25% increase in firm-specific wage premium on job change.

Overall, the literature is unambiguous on the existence and importance of informal job markets. The evidence suggests that referral effects differ between socioeconomic characteristics, which is why we differentiate between industries, age groups, nationality and education categories to quantify the extent.

⁶The author uses the European Community Household Panel (ECHP) from 1994-2001 and the NLSY from 1979-2000.

⁷The difference in data sources limits the exact comparability of these numbers.

3.3 Data

We employ registry data which are collected in the administrative processes of the German Federal Employment Agency (FEA, Bundesagentur für Arbeit) and maintained in the Integrated Employment Biographies (IEB) of the Institute of Employment Research (Institut für Arbeitsmarkt- und Berufsforschung, IAB). The IEB cover all employed persons who pay statutory social security contributions, all recipients of benefits from unemployment security (according to Social Code III) or from basic life support (according to Social Code II), all participants in active labor market policy, as well as all persons who approach FEA for job-search support. As our analysis focuses on estimating the probability of working together, we only use people in employment.

To ease computation we use data only for the Rhine-Ruhr region, Germany’s largest metropolitan area. It is a very densely populated area reflecting several aspects that also represent the whole of Germany. The area is diverse in its wealth and socioeconomic structure. It includes on the one hand prospering university cities like Bonn and on the other hand former heavy industry and mining centers, which have a high population of immigrants and a high proportion of unemployment like Gelsenkirchen.

The IAB Research Data Centre geo-coded both the work-place and the residential address of the IEB at June 30th 2008 (see Scholz et al. (2012)). Each person is assigned to a quadratic grid cell of 500m length to warrant anonymity compulsory in social security data provision. We use these grid cells as our basic definition of a neighborhood.

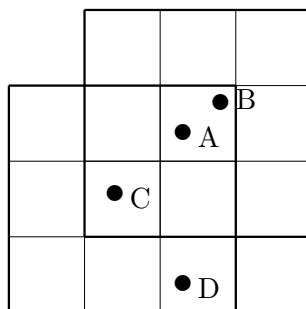


FIGURE 3.1: Defining Neighborhood by a regular Grid

Figure 3.1 illustrates the structure of the neighborhood definition: According to the exact address, every individual is assigned to a grid cell (the small squares in figure 3.1). Individuals A and B are immediate neighbors, whereas C shares what we will further on call a “super-neighborhood” with A and B. D lives within a super-neighborhood of C but not with A and B. In contrast to Bayer et al. (2008) and Schmutte (2015) who use predefined census blocks (neighborhoods) which belong to a fixed census block group (super-neighborhood). In our design, every grid cell (neighborhood) is the centroid of

a super-neighborhood and thus every grid cell belongs to several super-neighborhoods. Although the classification of neighborhoods and super-neighborhoods does not depend on geographic factors such as big roads or rivers, the flexible design guarantees an assignment for each grid cell to be the centroid of a super-neighborhood as well as part of the surrounding for all neighboring grid cells. We believe that this overlapping sampling scheme is an advantage as measured interaction is still very local but the conditioning surrounding is flexible.⁸ It may frequently happen that a person resides close to the outside border of the super-neighborhood when using a fixed definition as in Bayer et al. (2008). This causes scenarios, in which the adjacent reference group or super-neighborhood is closer to a person than the actual reference group the estimation strategy is conditioning on⁹. We use super-neighborhood fixed effects to deal with sorting on the basis of unobservables. As every grid cell belongs to 9 different super-neighborhoods, the influence of the remaining sorting within super-neighborhoods should be reduced substantially. Besides, using a neighborhood definition that is based on real distances rather than the number of people sharing a neighborhood (as it is the case for census blocks and census block groups) makes accounting for distances to workplaces and reflecting commuting behavior more realistic.

We observe roughly 4 million persons in our data, dispersed across 21,509 grid cells, who are aged 15-65 and participate in the labor force (without self-employed, civil servants and members of the armed forces). Of these persons, roughly 3.5 million are employees. To make computation feasible, we draw a 2% random sample from all employed persons and further denote these individuals as i . For all of these i , we match all possible neighbors j who either reside in the same neighborhood or super-neighborhood. In our analysis, a pair ij , who reside in the same super-neighborhood, refers to one observation. Our sample thus consists of 68,947 individuals i , 3.2 million of potential neighbors j and a sample of 179.7 million pairs ij .

Compared to working with (possibly larger) samples for both individuals and neighbors, the one-sided sampling has the advantage to enable conclusions on job referrals in the population more easily (with one-dimensional sampling probabilities, respectively univariate rather than bivariate cumulated densities). Figure B.2 in appendix B shows the distribution of neighborhood and super-neighborhood sizes. The mass of the neighborhood-size distribution lies in the range between 150 and 700 persons per grid cell; the average neighborhood size is around 320. However, the average pair is observed in a neighborhood with more than 900 inhabitants because larger neighborhoods have a higher probability to be represented in the sample, and a person in a large neighborhood

⁸This kind of mutually non-exclusive rolling-window delineation of super-neighborhoods is also a method of identifying neighborhood effects (Bramoullé et al., 2009).

⁹If we would consider only the lower block of grid cells in figure 3.1 as a fixed reference group, e.g. individual B would have D in its reference group, but none in the adjacent grid cells on its right or above.

has more neighbors.

The geographic scale in the IEB data set differs from that in the role model paper. While Bayer et al. (2008) use census blocks (which on average measure 160m of length) as a definition for neighborhoods, our neighborhoods are considerably larger measuring 500m×500m. Nevertheless, we believe that this extent is small enough to guarantee the possibility of people interacting with each other. For example the edge length of a grid cell corresponds to the standard distance between bus stops for medium and highly populated urban areas (see Köhler and Bertocchi, 2010). It approximates a walking distance of five minutes. Social interaction in a residential neighborhood can occur through meeting at points such as sport clubs, churches or elementary schools¹⁰.

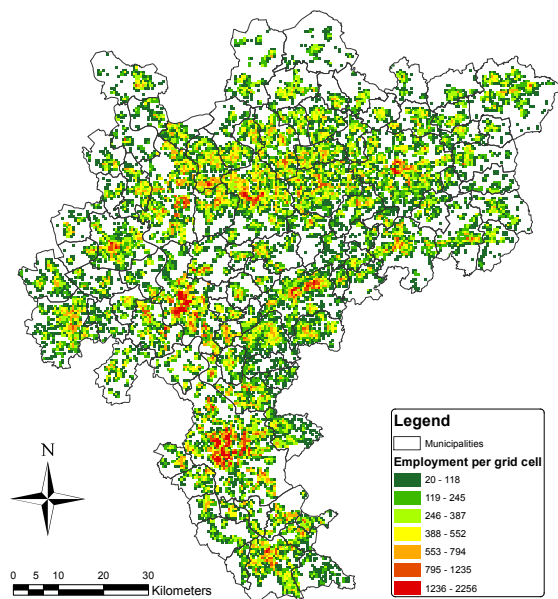


FIGURE 3.2: Employees in 500×500m grid cells in the Rhine-Ruhr metropolitan area

To illustrate neighborhood sizes and the geographic extent of the data set, figure 3.2 shows the dispersion of individuals in our sample across the Rhein-Ruhr area. Each dot represents a 500×500m grid cell and corresponds to our definition of a residential

¹⁰The Rhine-Ruhr area has 1,774 elementary schools, which differ in their dispersion: There is one elementary school per 2,522 inhabitants and a maximum 7,840 inhabitants per elementary school (data from the ministry of education in North Rhine-Westphalia at www.schulministerium.nrw.de). If e.g. parents meet when picking up their children and possibly form social contacts there, the extent of the draw area is larger than that of a residential neighborhood in our definition but smaller than a super-neighborhood.

neighborhood. The most densely populated grid cells (red areas) coincide with the area around Cologne in the South, Düsseldorf in the West and Dortmund in the East.

TABLE 3.1: Group Sizes in Population and Sample

Group	Population	Sample	Neighbors	Super-neighbors
		i	j w/ $R_{ij} = 1$	j w/ $R_{ij} = 0$
Male	0.5181	.5168	.5182	.5180
Age 15-24	0.0985	.0991	.0993	.0993
Age 25-34	0.2000	.2001	.2053	.2023
Age 35-54	0.5392	.5405	.5417	.5439
Age 55-65	0.1531	.1515	.1536	.1545
Unskilled	0.1485	.1477	.1509	.1493
Med. skilled	0.4750	.4780	.4722	.4752
Highskilled	0.0963	.0932	.0960	.0967
German	0.9032	.9018	.8992	.9023
Greek	0.0051	.0052	.0053	.0051
Italian	0.0086	.0083	.0089	.0086
Spanish	0.0019	.0021	.0020	.0020
Turkish	0.0352	.0355	.0372	.0357
Yugoslavian ^a	0.0104	.0105	.0108	.0104
From new EU ^a	0.0068	.0070	.0067	.0068
Other nationality	0.0288	.0296	.0298	.0290
Primary sector	0.0395	.0405	.0388	.0394
Manufacturing	0.1763	.1779	.1754	.1764
Construction	0.0458	.0455	.0455	.0457
TTC ^b	0.2622	.2643	.2630	.2620
Business Services	0.1757	.1746	.1765	.1758
Other Services	0.3005	.2972	.3007	.3008
# employees	3,459,941	68,947	3,169,180	3,397,929

Yugoslavian covers immigrants from the territory of former Yugoslavia (including Slovenia and Croatia); these are not included in the group of immigrants from new EU members (which come from Estonia, Latvia, Lithuania, Poland, Czech Republic, Slovakia, Hungary, Bulgaria, Romania, Malta and Cyprus).^b: Trade, Transportation and Communication (TTC).

In table 3.1, we compare socioeconomic groups in the population, in the sample, and in the neighborhoods and super-neighborhoods of the sampled persons. The groups considered here correspond to the covariates in our estimations. The countries and groups

of countries are the largest immigrant groups and those who traditionally came to Germany as guest-workers (southern European countries). Therefore we expect those groups to have formed particularly strong networks in Germany. As table 3.1 shows, the 2% sample is almost identical to the population with respect to observable characteristics.

3.4 Empirical Design

Our goal is to compare the propensity of working together when living in the same exact neighborhood with the propensity of working together when not living in the same neighborhood but only sharing a broader reference area, the super-neighborhood. Our empirical design allows to identify a social interaction effect based on within super-neighborhood variation. The baseline model can be summarized as follows:

$$W_{ij}^a = \rho_s + \alpha_0 R_{ij}^n + \varepsilon_{ij} \text{ with } a = \{n, f\} \quad (3.1)$$

W_{ij}^a is an indicator for individuals i and j (who live in the same super-neighborhood) to share the same workplace. W_{ij}^a takes on the values 0 or 100 so that parameters are directly interpretable as changes in percentage points. We differentiate W_{ij}^a over $a = \{n, f\}$: First, we follow Bayer et al. (2008) and set $W_{ij}^n = 100$ if a pair works in the same neighborhood n .¹¹ We refer to this case as *Referral to a neighborhood*. Second, we use exact information on the establishments. We define $W_{ij}^f = 100$ if a pair of individuals works at the same firm f and call this case *Referral to a firm*.

All specifications are estimated with heteroscedasticity and cluster robust standard errors¹². R_{ij}^n is equal to 1 if both i and j live in the same grid cell and zero otherwise. We can interpret α_0 , our social interaction effect, as the increase in probability of working together when sharing a neighborhood. ρ_s denotes a fixed effect for the super-neighborhood. It deals with sorting into residential location, a major issue of the neighborhood effects literature. Sorting causes selection bias due to correlation in unobservable factors in neighborhoods (such as amenities or the access to public transportation). α_0 can be identified as the social interaction effect if the two key assumptions are fulfilled: First, social interaction within a neighborhood is a local phenomenon. Second, individuals are able to choose their residential location freely across super-neighborhoods but are randomly located within, such that there is no correlation in unobservable characteristics affecting both work place and residential location within a super-neighborhood.

¹¹ A workplace area has size 1km×1km as to allow for manufacturing establishments (which occupy more space than most services) to be in the same area.

¹²Following Angrist and Pischke (2009), including robust standard errors deals with most of the problems when applying an LPM. Additional to the more straight forward interpretation of LPM estimating e.g. a Probit model would make computation infeasible given the extent of the data set.

To meet the requirement of the latter assumption, Bayer et al. (2008) argue that on a very local level, the housing market is comparably thin. When individuals are choosing their residential location, it may be hard to observe variation on a neighborhood level, whereas it is easier to see this variation between the larger super-neighborhoods. Furthermore, with 500m length a neighborhood is considerably small. It is not necessarily the case that one can find a suitable dwelling given an appropriate search period in a preferred small neighborhood, but rather has to look for something in a more spacious area (such as the super-neighborhood). Germans in general are less mobile than US Americans: 16% of Germans change their residence within two years and only 9% moved within a city (Böltkén et al., 2013), which gives rise to the assumption that the housing market is thin enough even within cities. Besides, the overlapping structure of our super-neighborhood design should additionally contribute to meet this criterion: Even if there is sorting within super-neighborhoods, sampling each small grid cell up to 9 times should substantially reduce the remaining sorting. This is a clear advantage compared to Bayer et al. (2008) and Schmutte (2015), who use predefined fixed census block groups as a reference group.

As outlined in Section 3.2, previous findings emphasize the differing importance of informal networks among socioeconomic groups. To account for those differences, we include individual characteristics.

$$W_{ij}^a = \rho_s + \beta'(X_i - \bar{X}) + (\alpha_0 + \alpha'_1(X_i - \bar{X}))R_{ij}^n + \varepsilon_{ij} \text{ with } a = \{n, f\} \quad (3.2)$$

We investigate how belonging to a socioeconomic group adds to the propensity of working together. α_1 depicts the effect of being part of a particular group and working together - a “one-sided” social interaction effect. To interpret the effect of sharing a neighborhood at the mean of the categorical variables X , we center all covariates around zero.¹³ We use categorical variables for personal characteristics such as sex, age groups¹⁴, skill groups¹⁵, categories of nationality, different industries, and a control for the size of the neighborhood. β can be interpreted as the baseline propensity of residing in the same super-neighborhood (but not sharing an immediate neighborhood) on working together for different characteristic groups (X_i).

$$W_{ij}^a = \rho_s + \beta'(X_{ij} - \bar{X}) + (\alpha_0 + \alpha'_1(X_{ij} - \bar{X}))R_{ij}^n + \varepsilon_{ij} \text{ with } a = \{n, f\} \quad (3.3)$$

¹³Wooldridge (2002) argues that subtracting the sample mean from each component allows identification of α_0 as the average treatment effect of R_{ij} on the dependent variable.

¹⁴Young adults from 15-24, career entrants aged 25-34, those established in the work force from 35-54 and senior workers between 55 and 65 years.

¹⁵Low skilled refers to lower secondary education with and without apprenticeship. Medium skilled individuals have higher secondary education (German “Abitur”), with and without apprenticeship. The high skilled group refers to individuals with a university degree.

In equation 3.3, we examine whether the propensity of working together varies with the characteristics of a pair (as opposed to the individual characteristic measured by equation 3.2). With this specification, we test whether e.g. more similar pairs are more likely to profit from social interaction and whether certain groups have higher probabilities to work together.

3.5 Results

Table 3.2 summarizes the results from our baseline model as presented in Section 3.4. Estimating unconditionally (without super-neighborhood fixed effects) gives an impression on the baseline probability of working together¹⁶: when residing in the same super-neighborhood the probability of working in the same neighborhood is 1.8% and 0.22% for working in the same firm. Estimating equations 3.1-3.3 can then be interpreted as an increase in this baseline probability by residing in the same neighborhood.

Column (1) corresponds to equation 1, where sharing a neighborhood is the single explanatory variable. The social interaction effect is positive and highly significant for both cases of $a = (n, f)$. Thus there is evidence for a positive impact of sharing a residential neighborhood on the propensity to work together in the same neighborhood as well as on the propensity to work in the same firm. For a referral to a neighborhood ($a = n$), the probability of working together is increased by 0.14 percentage points, which corresponds to an increase of 8%. Despite the different definition of neighborhoods the magnitude of the social interaction effect is similar compared to the 0.12 percentage points estimated by Bayer et al. (2008).

Interpreting the referral to a firm is a better indication for an actual job referral. The estimated absolute social interaction effect is somewhat smaller as compared to the referral to neighborhood effect, albeit still positive and highly significant. The probability for neighbors to work at the same firm increases by 0.07 percentage points. This is equivalent to a 30% increase in probability compared to the unconditional baseline probability which is much larger than in the case of a referral to a neighborhood. Therefore referrals to a firm not only reflect an actual referral more realistically, but also are economically more meaningful.

Columns (2) and (3) refer to equation 3.2 and equation 3.3, where we are interested in how the social interaction effect reacts first for different socioeconomic groups and second for pairs of socioeconomic groups. For expositional purpose, we only report joint significance in this table; full outputs are presented in the appendix. Noteworthy, the social interaction effect α_0 is relatively stable across specifications. Column (2) shows

¹⁶Here, we estimate $W_{ij}^a = \alpha_0 + \alpha_1 R_{ij}^n + \varepsilon_{ij}$ and interpret α_0 as the baseline probability of working together when sharing the same super-neighborhood.

TABLE 3.2: Estimation of Referral Effects

Variable	Referral to neighborhood ($a = n$)			Referral to firm ($a = f$)				
	No FE	(1)	(2)	(3)	No FE	(1)	(2)	(3)
Constant	1.7982*** (.0011)	1.7941*** (.0037)	2.2207*** (.4000)	2.0837*** (.3811)	.2169*** (.0004)	.2189*** (.0038)	.2347*** (.1063)	-1196 (.1054)
R_{ij}	.1111*** (.0027)	.1368*** (.0238)	.1432*** (.0238)	.1238*** (.0185)	.0868*** (.0010)	.0746*** (.0240)	.0784*** (.0241)	.0605*** (.0182)
Relative in-crease		[7.61%]	[7.96%]	[6.88%]		[34.39%]	[36.15%]	[27.89%]
Sex	—	—	X_i^{***} $R_{ij}X_i^{***}$	X_{ij}^{***} $R_{ij}X_{ij}^{***}$	—	—	X_i^{***} $R_{ij}X_i^{***}$	X_{ij}^{***} $R_{ij}X_{ij}^{***}$
Qualification	—	—	X_i^{***} $R_{ij}X_i^{***}$	X_{ij}^{***} $R_{ij}X_{ij}^{***}$	—	—	X_i^{***} $R_{ij}X_i^{***}$	X_{ij}^{***} $R_{ij}X_{ij}^{***}$
Age group	—	—	X_i^{***} $R_{ij}X_i^{***}$	X_{ij}^{***} $R_{ij}X_{ij}^{***}$	—	—	X_i^{***} $R_{ij}X_i^{***}$	X_{ij}^{***} $R_{ij}X_{ij}^{***}$
Ethnicity	—	—	X_i^{***} $R_{ij}X_i^{***}$	X_{ij}^{***} $R_{ij}X_{ij}^{***}$	—	—	X_i^{***} $R_{ij}X_i^{***}$	X_{ij}^{***} $R_{ij}X_{ij}^{***}$
Industry	—	—	X_i^{***} $R_{ij}X_i^{***}$	X_{ij}^{***} $R_{ij}X_{ij}^{***}$	—	—	X_i^{***} $R_{ij}X_i^{***}$	X_{ij}^{***} $R_{ij}X_{ij}^{***}$
Incoresize	—	—	X_i^{***} $R_{ij}X_i^{***}$	X_{ij}^{***} $R_{ij}X_{ij}^{***}$	—	—	X_i^{***} $R_{ij}X_i^{***}$	X_{ij}^{***} $R_{ij}X_{ij}^{***}$
σ_u	—	5.2934	36.6795	31.1610	—	2.5382	5.1042	4.6731
σ_ε	13.351	13.2881	13.2821	13.2703	4.7956	4.7654	4.7623	4.7485
# pairs ij	179.9 Mio	197.9 Mio	197.9Mio	197.9 Mio	197.9 Mio	197.9 Mio	197.9 Mio	197.9 Mio
# groups	—	11,757	10,159	10,159	—	11,757	10,159	10,159
Corr(u,Xb)	—	-.0072	-.0042	-.9996	—	.0083	.0249	-.9984

Heteroscedasticity-consistent standard errors in parentheses. */**/*** mark significance at the 90%/95%/99% confidence level. Asterisks at the control variables and interaction terms mark significant differences between the groups. Reference is a female German in the age between 35 and 54 with unknown qualification working in Manufacturing, or a pair of two women with these characteristics, respectively. Coefficient estimates are reported in the appendix. $R_{ij}=1$ if two individuals share the same direct neighborhood.

the one-sided interaction effect. Here, only some of the interactions are jointly significant: There is no statistically significant differential effect of sharing a neighborhood varying by qualification, age group or gender both for referrals to neighborhoods and to firms. An individual's own ethnicity¹⁷ and industry¹⁸ generate significant variation in the referral effect. The larger the neighborhood i lives in, the smaller the referral effect; this corresponds to the decreasing likelihood of interaction the more people live in one neighborhood.

Column (3) describes how pairs of certain groups interact in residential neighborhoods. X_{ij} describes the propensity to work together when sharing a super-neighborhood: We find higher propensities for young and old pairs of workers, as well as unskilled pairs and matches for several industry sectors. In contrast, there is almost no effect for ethnic groups. Again the interaction term determines the local referral effect. Apart from age groups, the impact of all categories are jointly significant which means that grouping pairs with respect to socioeconomic categories at least plays some role for job referrals. The interaction effects (α_1 in equation 3.3) can be interpreted as the additional effect of being both in the same socioeconomic group and sharing a neighborhood. There are no big differences across gender and age groups (meaning that the interaction effects are either small or insignificant). Consistent with the literature on informal job markets (e.g. Hellerstein et al. (2011) or Kramarz and Nordström Skans (2014) among others), pairs of unskilled workers have a comparatively high propensity to work together both at the same neighborhood and at the same firm. For different ethnic groups the effect varies, too: Especially for people from the new EU countries, the probability to work together increases by over 20% as compared to Germans (the reference group) both for referrals to neighborhoods and referrals to firms. Also Italians and people from former Yugoslavia show stronger referral effects. In contrast, albeit being the biggest migrant group in Germany, Turkish do not seem to behave differently from Germans, with the interaction effect being insignificant. For the different types of industries¹⁹, the propensity to work together is increased in a similar way across groups. The size of the residential neighborhood of pairs seems to have no effect on working together; it has a significant negative effect on the interaction (the referral), however. We interpret this as decreasing probability to meet when living in a higher populated neighborhood, in

¹⁷The effect differs between referrals to neighborhoods, where Greeks have the most significant increase in probability of working together, and referrals to neighborhoods, where Turks seem to profit the most from referral effects. For all other groups, the effects are positive but rather noisy.

¹⁸Compared to women working in manufacturing, working in all other industry sectors has a negative effect on working together when sharing a neighborhood, with business related services having the largest and most significant effect.

¹⁹An exception is the Primary Sector. Here the increase in propensity to work together can probably be accounted for – at least to some extent – by disproportionately many people living very close to their workplace.

line with Calvó-Armengol and Zenou (2005)²⁰.

We use a linear model for computational reasons. A potential concern on the specification is estimating a linear model does not accurately reflect potential non-linearities in the propensity of working together and therefore overstates the true network effect. We therefore calculate linear predictions of \hat{W}_{ij} using equation 3.2 and 3.3. We find the mean and median predicted probability to be close to the baseline probability of working together in a super-neighborhood. We interpret this as an indication for an LPM being a suitable specification.

3.6 Robustness

We argue that the key assumption for identification, no correlation in unobservables affecting work location within a super-neighborhood, is reasonable. Following Altonji et al. (2005), selectivity in observables is proportional to selectivity in unobservables and an indication of sorting. Therefore, we first analyze the sorting behavior with respect to observable characteristics. We compute correlations of observable characteristics (age groups, gender, nationality groups, skill groups and industry groups) for both pairs that reside in a neighborhood together and for pairs who share a super-neighborhood but are not immediate neighbors. The correlations are expressed as $E(D_i \frac{1}{n_i} \sum_j D_j) = E(X_{ij})$ and characterized by the expected value of observing two individuals i and j belonging to the same group D^{21} , and test whether they differ between neighbors and super-neighbors. Table B.1 in appendix B presents correlations on the basis of observables. We see no systematic differences, with the super-neighborhood having slightly less correlations. This suggests sorting on the basis of observables but no difference in the patterns of sorting between neighborhoods and super-neighborhoods. Apart from that, especially Turkish, people from former Yugoslavia and the new EU countries sort themselves together into neighborhoods. In contrast, immigrants from other southern European countries tend to sort away from each other. This is remarkable considering the interpretation of the interaction effects presented above: Turkish, who seem to sort themselves together do not tend to be more likely to work together. In contrast, Italians and Spanish who have an increased probability to work together tend to sort away from each other²². This indicates that although we find some sorting in observables, it does not seem to bias our interaction estimates systematically.

²⁰Calvó-Armengol and Zenou (2005) show in a matching framework, that the probability of finding a job increases with network size up to a critical value, where the job finding probability decreases.

²¹Therefore some of the correlations are very high just because the group is comparatively big, which is why the probability to be matched into a pair with your own group is high.

²²The very high positive effect for new EU migrants, however, seems to be inflated by positive sorting bias. Nevertheless, as it is big and statistically highly significant, we believe that referrals should still play a role for them.

Second, we analyze whether there is sorting within super-neighborhoods with respect to unobservables. The residuals from estimating equation 3.2 represent everything which is unobservable with respect to the choice of residential and working location. We therefore use them as a proxy for sorting on the basis of unobservables. By construction, the residuals should have an average value of zero on the basis of super-neighborhoods. We compare the mean residuals for those pairs sharing a neighborhood (i.e. $R_{ij} = 1$) within each super-neighborhood with those sharing a super-neighborhood but not its core ($R_{ij} = 0$). This gives a direct test for sorting on the basis of unobservables. For the estimation of a referral to a neighborhood in specification (1) (estimation of equation (3.1)), the 1st percentile of the neighborhood-specific averages is -1.11 and the 99th percentile is 2.33, for referrals to a firm none of these values is far away from zero, as compared to the variation of the fixed effects (see table 3.2). If we control for covariates in specifications 2 and 3, these averages become even closer to zero. Therefore, we conclude that we can reject sorting on the basis of unobservables affecting both workplace and residential location within super-neighborhoods. This means that our empirical design deals successfully with self selection of residential location, the most important issue in identification of neighborhood effects.

3.6.1 Reverse Causality

Another concern is the possibility of reverse causality. Instead of neighbors referring each other for a job, what we observe could also be the result of coworkers recommending each other their residential neighborhood. To check which direction of the effect is the most plausible, we select four different subsamples and re-estimate equation 3.1. The results are presented in table 3.3. As the IEB is only geocoded in 2008, we have to rely on geographic information in form of zip codes two years prior to our main sample, in 2006. Zip codes refer to districts within cities. The residential areas in this specification are larger than those of our main specification but still represent movements within cities.

First, in a subsample of “residential stayers”²³, the estimate for a referral to a neighborhood ($a = n$) rises slightly to 0.1552 percentage points and .0828 for referrals to a firm ($a = f$). The constant rises, too, which is associated with an overall increase in probability of working together. This is due to the fact that restricting the sample to only residential stayers mainly excludes pairs not working together. The relative increase²⁴ is very similar for both types of referrals being slightly smaller than in the baseline specification with the whole sample.

²³People who have been living in the same zip code area for the last two years.

²⁴As we use different subsamples for this exercise, we use the constants for each estimation as a baseline probability to calculate the relative increase here.

TABLE 3.3: Referral Effects amongst Job Movers and Residential Stayers

Variable	Res. stayers ($a = n$)	Res. stayers ($a = f$)	Job movers ($a = n$)	Job movers ($a = f$)	Res. stayers with job move ($a = n$)	Res. stayers with job move ($a = f$)	Housing referral ($a = n$)	Housing referral ($a = f$)
Constant	2.095*** (.0036)	.2756*** (.0035)	1.8667*** (.0054)	.1593*** (.0055)	1.8066*** (.0051)	.1513*** (.0051)	2.3407*** (.0057)	.3278*** (.0047)
R_{ij}	.1552*** (.0228)	.0828*** (.0223)	.1351*** (.0347)	.0796*** (.0355)	.1183*** (.0326)	.0739** (.0329)	.1861*** (.0368)	.0980*** (.0305)
Relative in-crease	[7.41%]	[30.04%]	[7.23%]	[49.97%]	[6.55%]	[48.84%]	[7.95%]	[29.89%]
σ_u	5.3345	2.5287	3.5007	.6615	3.2159	.6691	3.2471	1.4941
σ_ε	14.3187	5.3321	13.5379	4.0968	13.3088	3.9935	15.0251	5.7943
# pairs	102.9 Mio	102.9 Mio	105.3 Mio	105.3 Mio	62.1 Mio	62.1 Mio	9.6 Mio	9.6 Mio
# groups	10,662	10,662	10,792	10,792	10,165	10,165	9,262	9,262
Corr(u,Xb)	-.0079	.0073	-.0059	.0085	-.0070	.0063	-.0045	.0057

Heteroscedasticity-consistent standard errors in parentheses. [] gives increase in probability of working together. * / ** / *** mark significance at the 90%/95%/99% confidence level. Residential stayers: Pairs who both live in the same zip code area and have lived there for at least two years. Job movers: Pairs of which at least one has moved her workplace across zip code districts within the previous two years. Housing referral: Pairs both working in same zip code area for two years, one of them has changed residential location.

Second, we use a subset of “job movers”²⁵: We select only pairs of which one individual has changed the workplace (defined as the zip code where an individual works). For both referrals to a neighborhood and to a firm the absolute effect is very close to the one estimated with the whole sample whereas there is an increase in the relative probability for a referral to a firm.

Third, we select individuals, who have all lived in the same zip code in the last two years and use only pairs of which one individual has changed the working location, i.e. “residential stayers with a job move”. Here, the absolute effect decreases slightly for both kinds of referrals but remains statistically significant²⁶. It is worth noting that while there is a slight decrease in the relative effect for referrals to a neighborhood, the relative effect for firms increases to 49%. In this group the observed correlation is most likely to be caused by a job referral. One of the pair has been searching a job in the previous two years while both stayed at their joint neighborhood. The estimated absolute referral effects in the baseline specification and in this restricted sample are not statistically different from each other (for both cases of $a = \{n, f\}$). This again makes us confident that the social interaction effect we find is indeed a job referral effect.

Finally, we select a subsample where it is most likely to observe a referral on the housing market: We use pairs of which one individual has lived in the zip code area for the last two years while the other has changed residence. At the same time, both individuals have worked in the same zip code area during that period. So within the pair ij , there is one change in residential location but no change in employment for both. In this circumstance the estimated social interaction effect is most likely caused by coworkers exchanging information on the housing market. The absolute effect of R_{ij} is increased and highly significant both for $a = n$ and $a = f$. Nevertheless the sample size is considerably smaller than in all other cases and the sample seems to be inherently different from those before: The magnitude of the constant suggests, that by selecting this specific subsample, we exclude primarily individuals not working together (i.e. zeros for W_{ij}), which could be a reason why the estimated absolute interaction effect is bigger than in the estimation with the whole sample. For referrals to a neighborhood, the baseline probability of working together when sharing a super-neighborhood increases by 15% compared to the estimation with the whole sample. For referrals to a firm it even increases by 50%. Apart from that, people in this subsample should differ from those in the whole sample, as we explicitly select individuals with a stable employment. The relative increase is larger in case of a referral to a neighborhood but with 29.89% for a

²⁵This specification includes individuals who move to find a new job. It is not directly interpretable as a exclusion of reverse causality but should give us a more precise feeling for the magnitude of the third effect.

²⁶This subsample differs from the whole sample, which is why we should not suspect the effect to be as big as that for the whole sample: This is in line with Bayer et al. (2008), who find a social interaction effect of 0.09 percentage points for job movers.

referral to a firm considerably smaller, especially when compared to the case where a job market referral is most likely.

Although we can not rule out reverse causality completely, we find evidence for a referral effect in a case the job referral is most likely. Besides, we want to emphasize the importance of distinguishing between referrals to a neighborhood and a firm: Not only does the latter reflect theoretical considerations of a referral effect more closely (see Section 3.2), but this specification also seems to be more stable and less susceptible to bias than the case of a referral to a neighborhood.

3.6.2 Random Reassignment to Jobs

Is it possible that the correlation we observe is induced by something other than referrals by neighbors? Workplaces are neither evenly nor randomly allocated over space. They follow a certain structure because firms settle up more frequently in the central business district, subcentral business districts, or particular business zones (see e.g. Fujita et al. (1999) for an overview). As a consequence, a certain correlation with regard to workplaces may arise because people optimize their commuting distance. In order to disentangle this spurious correlation from the correlation due to job referrals, we randomly reassign a workplace neighborhood to persons i according to the workplace probabilities in their super-neighborhood. To do so, we determine for each super-neighborhood s the specific relative frequencies (i.e. the probabilities) for each workplace neighborhood, $p_{n|s}$, with cumulated frequencies $F_{n|s} = \int \cup_{m \in [1, \dots, n]} p_{m|s}$. The frequencies add up to the unit interval as $\int \cup_{n \in [1, \dots, N]} p_{n|s} = 1$. Then we draw from a uniform distribution for each person i . The realization of this draw corresponds to a unique workplace n specific partition on the unit interval (as $\{u_i \in (F_{n-1|s}, F_{n|s}]\} \mapsto n$) which determines a counterfactual workplace for each person i . Then we can construct a new variable for the hypothetical workplace coincidence, \tilde{W}_{ij}^n , and re-estimate equation 3.1:

$$\tilde{W}_{ij}^n = \rho_s + \alpha_0 R_{ij} + \varepsilon_{ij} \quad (3.4)$$

The spurious correlation due to clusters in employment is positive and statistically highly significant. Nevertheless, the magnitude of both the absolute and relative effect is small compared to the baseline specification in table 3.2. All in all, this indicates that what we measure as a referral effect using Bayer et al. (2008)'s design (referral to a neighborhood) is probably slightly inflated by clusters in employment. Again we emphasize the superiority of our additional specification with referrals to a firm.

TABLE 3.4: Baseline Estimation for artificial Workplaces

Variable	\tilde{W}_{ij}^n
Constant	1.8195*** (.0016)
R_{ij}	.0278*** (.0104)
Relative increase	[1.52%]
σ_u	2.1095
σ_ε	13.2895
# pairs	155.7 Mio
# groups	11,376

Heteroscedasticity-consistent standard errors in parentheses. [] gives increase in probability of working together. */**/** mark significance at the 90%/95%/99% confidence level. \tilde{W}_{ij}^n denotes the counterfactual probability of working together. Note: Here we only estimate artificial referrals to a neighborhood.

TABLE 3.5: Firm Size Effects for Referrals to a Neighborhood

Variable	Small firms	Small f. males	Medium firms	Large firms
Constant	1.8344*** (.0018)	1.764*** (.0025)	1.4818*** (.0016)	2.0055*** (.0074)
R_{ij}	.1300*** (.0115)	.1062*** (.0160)	.0756*** (.0105)	.1490*** (.0477)
Relative increase	[7.1%]	[9.0%]	[5.1%]	[7.4%]
σ_u	3.3893	1.6823	3.4128	4.3674
σ_ε	13.4182	10.7988	12.0897	13.9568
# pairs	74.2 Mio	18.1 Mio	47.3 Mio	59.8 Mio
# groups	10,785	9,343	10,474	10,535
$Corr(u, Xb)$	-.0090	-.0052	-.0058	.0048

Heteroscedasticity-consistent standard errors in parentheses. [] gives increase in probability of working together */**/** mark significance at the 90%/95%/99% confidence level. Small firms: < 50 employees, medium: 50-249 employees, large: ≥ 250 employees.

3.6.3 Firm size effects

Another issue closely related is a potential firm size effect: Does the high correlation of living together and working together come from people working in large establishments? To rule out another form of spurious correlation, we estimate our baseline specification for three different subsets. First, we re-estimate equation 3.1 for pairs of individuals of whom the neighbors j are working in small firms (less than 50 employees). The second category are medium sized firms, where j work in establishments with 50 to less than 250 employees. Third we look at a subset of neighbors who work in large establishments, employing more than 250 people. Again we differentiate between referrals to a neighborhood and to a firm.

Table 3.5 shows the probability of working in the same neighborhood when j works in different sizes of establishments. We observe no systematic difference by firm size. Both

TABLE 3.6: Firm Size Effects for Referrals to a Firm

Variable	Small firms	Small f. males	Medium firms	Large firms
Constant	.0156*** (.0001)	.01711*** (.0004)	.0880*** (.0013)	.5788*** (.0074)
R_{ij}	.0217*** (.0011)	.0233*** (.0023)	.0418*** (.0081)	.1275*** (.0472)
Relative increase	[139.9%]	[136.3%]	[47.6%]	[22.02%]
σ_u	1.7040	.5687	1.0772	4.3674
σ_ε	1.3745	1.4367	3.0664	13.9568
# pairs	74.2 Mio	18.1 Mio	47.3 Mio	59.8 Mio
# groups	10,785	9,343	10,474	10,535
$Corr(u, Xb)$.0036	.0061	.0162	.0048

Heteroscedasticity-consistent standard errors in parentheses. [] gives increase in probability of working together. */**/** mark significance at the 90%/95%/99% confidence level. Small firms: < 50 employees, medium: 50-249 employees, large: ≥ 250 employees.

absolute and relative increases in probability are fairly similar to our baseline specification, with medium firms having the smallest effect. Working in the same neighborhood with people from one's super-neighborhood (constant) is most likely for large firms, an indication of a size effect. Regarding the relative effect, the magnitude is in line with our other specifications.

We are concerned of biased results in the small size category, because here we should have many family businesses, where people work and live together as a result of being part of the same household. As we cannot identify household affiliation in our data, we estimate the equation for small firms for males only. For both specifications $a = n$ and $a = f$ the results do not differ significantly from the unrestricted sample. More interestingly, table 3.6 indicates huge and significant increases in the relative probability of working in the same small firm. Excluding females from the sample does not change this result, but leaves both absolute and relative effect unchanged. When j is working in a medium size establishment, i 's absolute probability of working with j is comparable to that in the whole sample. In case of large firms, the relative effects is smallest and least precisely estimated²⁷. Although we do not want to stress causal interpretation here, as our subsets are selective, we see these results as a clear indication that our estimates are not driven by a pure size effect meaning people are only working with their neighbors because they all work in big firms. In contrast, small establishments seem to be best suitable for referring someone from your network, maybe because employers rely more heavily on informal referrals or because information flows easier in small plants. This result is also in line with Hellerstein et al. (2011), Ioannides and Loury (2004) and Pellizzari (2010).

²⁷ Albeit being significant on a 1% level, the 95% confidence interval of R_{ij} goes from 0.0349 to 0.2201, which is large considering the sample size.

TABLE 3.7: Baseline Estimation excluding Short Distance Commuters

Variable	$a = n$	$a = f$
Constant	1.9528*** (.0040)	.2390*** (.0041)
R_{ij}	.1298*** (.0269)	.0787*** (.0262)
Relative increase	[6.64%]	[32.92%]
σ_u	4.9131	2.2322
σ_ε	13.8424	4.9754
# pairs	154.2 Mio	154.2 Mio
# groups	11,325	11,325

Heteroscedasticity-consistent standard errors in parentheses. [] gives increase in probability of working together. */**/** mark significance at the 90%/95%/99% confidence level.

3.6.4 Commuting

We now explicitly address the effect of commuting behavior. We are concerned that our measured effect could be driven by a disproportionately high number of short distance commuters. They locate close to their work place and hence have a high probability of working with their neighbors. Therefore, we exclude everyone working in the same zip code area they live in. We re-estimate equation 3.1 with this restricted sample to test whether the coefficient of social interaction α_0 differs significantly from that in the full sample. The sample size drops moderately to 154.2 million pairs ij , which means our restriction does not reduce the data set fundamentally. Furthermore, both the constant and the social interaction effect remain at a comparable level. The baseline probability of working together when sharing the same super-neighborhood (constant) is slightly higher both for referrals to a neighborhood and referrals to a firm. The absolute effect of R_{ij} in contrast is slightly lower for a referral to a neighborhood, but still in a very similar range with .13 versus .14 using the whole sample. For referrals to a firm, the effect is marginally higher as compared to the estimation with the whole sample (.0787 versus .0746). Over all, the results stay very much the same, which suggests that short distance commuters are not driving the referral effect and we do not observe a spurious correlation here.

Even if people do not systematically work together because they minimize their commuting distance (see Section 3.6.2), they could just as well be minimizing their commuting time. Workers could sort close to access to junctions of public transportation which likewise can lead to spurious correlation. We amend our data set with georeferenced

data²⁸ of all S-Bahn²⁹ stations of the linked transport system Rhine-Ruhr³⁰ (VRR), which extends to the greatest part of the Rhine-Ruhr metropolitan area³¹. We investigate whether the probability of working together and living together is increased, when people work close to access in public transportation. We look at whether such “networks of commuters” increase the probability of working together when sharing the same neighborhood. To check this, we estimate

$$W_{ij}^a = \rho_s + \beta'(S_j - \bar{S}) + (\alpha_0 + \alpha_1'(S_j - \bar{S}))R_{ij}^n + \varepsilon_{ij} \text{ with } a = \{n, f\} \quad (3.5)$$

where $S_j = 1$ if individual j works in a neighborhood that has an S-Bahn stop. A positive value for β indicates an increase in the baseline probability of working together at a workplace which is easier to access. The social interaction effect α_1 is interpretable as “network of commuters” effect, as it reflects the increase in probability of working together when living in the same neighborhood if the commuting destination has an S-Bahn stop³².

Table 3.8 shows the results of estimating equation 3.5. (1) indicates a subsample where all ij work in the VRR area, (2) restricts the full data set to all ij living in the VRR area and (3) uses the intersection of (1) and (2) where ij both work and live in the VRR area. Using only the VRR region (instead of the whole Rhine-Ruhr area) still leaves us with a large dataset.

When estimating the probability of working in the same neighborhood ($a = n$), we find an increase in the baseline probability of working together in the same neighborhood if that neighborhood has access to an S-Bahn station ($S_j = 1$). However, in the case of referrals to a firm, the effect of S_j is statistically not different from zero in any of the specifications.

The social interaction effect without access to a public transportation network R_{ij} is a bit smaller than in the baseline specification both for referrals to a neighborhood and referrals to a firm, but still in a comparable range. What is more interesting though is the interaction effect $S_j \times R_{ij}$: The probability of pairs to work in the same neighborhood which has an S-Bahn stop is negative. This means, an S-Bahn stop at the workplace actually reduces the probability of working together with your neighbor, which is in

²⁸According to the addresses of S-Bahn stations, we assign them to their respective neighborhood cells. If a station lies within multiple cells, we use the geometric centroid of the station to assign it to the corresponding neighborhood.

²⁹S-Bahn is a German commuter rail network that serves within city centers and suburbs nearby big cities. The S-Bahn is usually faster and serves a larger area than the U-Bahn or metro.

³⁰Verkehrsverbund Rhine-Ruhr (VRR). See <http://www.vrr.de/en/> for further information.

³¹The VRR encompasses with the Ruhr region, the Niederrhein, Wuppertal, Remscheid, Solingen and Düsseldorf the biggest part of the Rhine Ruhr area. Only the South (with Cologne and Bonn) and the North East are not included in this public transportation network.

³²Controlling in the same fashion for S-Bahn stops at the place of residence is impossible as the information on the presence of a station (within a super-neighborhood) will be absorbed by the super-neighborhood fixed effect.

TABLE 3.8: Referral Effects including Public Transportation Stations

Variable	$(a = n)$			$(a = f)$		
	(1)	(2)	(3)	(1)	(2)	(3)
Constant	2.2175*** (.0103)	1.7112*** (.0078)	2.2330*** (.0104)	.2562*** (.0018)	.2151*** (.0013)	.2537*** (.0017)
R_{ij}	.1020*** (.0180)	.0810*** (.0148)	.0994*** (.0182)	.0620*** (.0077)	.0495*** (.0060)	.0593*** (.0074)
S_i	1.4802*** (.1300)	1.2864*** (.1081)	1.4811*** (.1323)	-.0171 (.0180)	-.0170 (.0153)	-.0155 (.0183)
$S_i \times R_{ij}$	-.1393*** (.0527)	-.1137*** (.0436)	-.1382** (.0535)	-.0064 (.0185)	-.0037 (.0190)	-.0016 (.0185)
σ_u	5.2746	5.3047	5.3301	2.7585	2.8480	2.7319
σ_ε	15.0916	13.3138	15.1384	5.1216	4.6908	5.0942
# pairs	93.8 Mio	125.3 Mio	92.4 Mio	93.8 Mio	125.3 Mio	92.4 Mio
# groups	8,704	7,727	7,238	8,704	7,727	7,238
$Corr(u, Xb)$	-.0445	-.0408	-.0442	.0048	.0053	.0041

Heteroscedasticity-consistent standard errors in parentheses. [] gives increase in probability of working together. */**/** mark significance at the 90%/95%/99% confidence level. (1) is a subsample of ij with workplace in VRR, (2) indicates residence of ij in VRR and (3) shows all ij with both workplace and residence in this area.

contrast to the interpretation of the “network of commuters”. Nevertheless we should interpret this result with caution: Although two out of three specifications are significant at a 1% level, the measurement of the effects is imprecise with rather big confidence intervals compared to other specifications. Estimating the probability of working in the same establishment with access to public transportation and living in the same neighborhood also yields negative signs, but none of the specifications are statistically different from zero. A reason for the negative sign could be the fact that S-Bahn stations take up a lot of space which reduces the probability to work in a 500×500m grid cell together, where there is also a public transport station. Overall our analyses do not support the hypothesis that commuting is a source of bias to our estimates of a residence based referral effect and that – especially in our preferred specification with $a = f$ – we find very similar results when excluding short distance commuters and no significant results when conditioning on access to public transportation.

3.7 Discussion

Most of the empirical work on neighborhood and referral effects so far has been on US American data; in contrast, we look at residence based referral effects for the Rhine-Ruhr area, one of the biggest agglomerations in Europe. We use the empirical design proposed by Bayer et al. (2008) to compare propensities to work together when sharing an immediate neighborhood while holding the surrounding neighboring area constant. This allows us to identify a social interaction effect using the within variation of the so-called super-neighborhoods.

The results of our baseline specification are very similar to those for the Boston metropolitan area: We significantly estimate the probability to work with a neighbor to be 0.14 percentage points while Bayer et al. (2008) find the effect to be 0.12 percentage points. So the first question whether the extent of referral effects based on residential location differs for a European country as compared to the US can be denied: Although we use a different definition of neighborhood and super-neighborhood, we find very similar results.

Novel geo-coded data further allow us to differentiate referral effects as a “referral to firm” effect which is a much more precise measure consistent with theoretical considerations on job referrals. The absolute effect is about 0.07 percentage points and somewhat smaller than the referral to a neighborhood. However, the relative increase is with over 30% as compared to 8% larger and hence economically more meaningful. We interpret this referral to a firm as the more precise measure for job referrals, as information on available jobs is restricted mostly to one’s own firm. Additionally, the effect seems to be even more stable across specifications. Hence, we argue that the previous literature

understates actual network effects.

The referral effects are even stronger for similar pairs especially of the same nationality. This finding is in line with the literature of immigration and integration and also supports my interpretation from chapter 2: E.g. Glitz (2014) shows substantial immigrant segregation both in workplaces and in residential location in Germany, which varies for groups of immigrants. Segregation in residential location seems to be independent of qualification, whereas segregation in workplaces is more present for low qualified immigrants. Here, we can differentiate residential locations in much more detail due to the geocoded data and we see that some ethnicities sort themselves together (see table B.1) and also show an increased propensity to work together. Therefore the presumption of chapter 2 and its effects are supported by our findings in this chapter.

Our estimates for referral effects are stable across various specifications such that we can exclude several other explanations besides a job referral effect. We find that there is no sorting on the basis of unobservables within super-neighborhoods. This means that including the fixed effects should deal with the issue of self selection into residential location, the greatest threat to empirical studies on neighborhood effects.

Although we cannot rule out completely the possibility of a bias in our estimated referral effect due to simultaneity, we argue that it is very plausible that what we observe accounts for an actual referral effect on the job market. We find very similar results for a subset of individuals, for whom job referrals are most likely.

We amend the previous literature by explicitly checking for clusters in employment and find positive and significant spurious correlation of 0.03 percentage points due to the geographical distribution of workplaces. However, the greater portion can be attributed to an actual referral effect.

We find no evidence that our estimates of referral effects are driven by many people working in large firms. In contrast, we find that the relative effect is extremely high for referrals to a small firm, which suggests that for small businesses informal job market channels are most important (which is in line with e.g. Hellerstein et al. (2011) Ioannides and Loury (2004) and Pellizzari (2010)).

Finally, we analyze the role of commuting on our referral effect. Neither short distance commuters nor networks of commuters seem to be the drivers of our measured referral effect.

Chapter 4

The Distributional Effect of Commuting Subsidies

4.1 Introduction

Tax laws in most OECD countries foresee some kind of tax break for commuting. Underlying these regulations are usually both efficiency and equity considerations. On the one hand, commuting subsidies are intended to increase efficiency in the labor market by encouraging workers to augment their radius of job search and to commute further for a better match (Borck and Wrede (2009)).¹ On the other hand, equity considerations require that workers willing to accept longer commuting distances should not be disadvantaged financially (Borck and Wrede (2005)).

Compared to the efficiency aspect, little is known about the distributional consequences of commuting subsidies.² In this paper we provide new and consistent evidence on the distributional effects of commuting subsidies. Using precise information on commuting distances of about two million workers, we draw on a unique policy reform in Germany. Commuting subsidies were reduced substantially in 2007 for workers commuting 14 kilometers or more while leaving workers with lower commuting distances unaffected. With 2.5 billion Euro of additional tax revenues annually (Donges et al., 2008), these

¹Commuting subsidies are usually designed as deductions of commuting expenses from taxable income. As such, they offset negative effects from income tax on job search and commuting decisions (Richter et al., 2004). In a simple example, let Δw be the wage premium for commuting and c the commuting cost. In the absence of taxation commuting will take place if $\Delta w - c > 0$. Sufficiently high income taxes t will inhibit commuting since $\Delta w(1 - t) - c < 0$. If, however, commuting is tax deductible, every efficient job match will be achieved even under taxation since $(\Delta w - c)(1 - t) > 0$ holds as long as $\Delta w - c > 0$.

²A substantial body of theoretical literature examines how commuting subsidies should be designed in order to reach an efficient level of job search and commuting (see, e.g., Wrede (2001) and Richter (2006)). Weiss (2009) and Boehm (2013) both provide empirical evidence that workers commute longer distances if they can offset commuting expenses against income tax.

abrupt changes in the tax regime were not only substantial in size but have also led to a large shift of a major kink in the tax scheme. In our identification approach we exploit the exogenous variation in commuting subsidies induced by the reform. Using a difference-in-differences strategy we first examine whether workers are compensated by their employer for the net wage losses they incur as a result of the partial repeal of commuting subsidies. Empirically, the existence and the size of compensation payments are ex-ante unclear, as they depend on multiple factors: The mobility of workers and firms and the market structure on the demand and supply side (Manning (2003)), the level of information that firms possess (Zenou (2006), Ross and Zenou (2008)), the propensity of workers to shirk (van Ommeren and Gutiérrez-i Puigarnau (2011)), the relative bargaining power of workers (Rupert et al. (2009)) as well as the extent to which wages are flexible enough to adjust to worker-specific circumstances (Baldry (1998)).

If workers are not (fully) compensated for commuting costs by their employers, commuting subsidies effectively reduce the financial burden of commuters. The question from an equity perspective is then how the benefits from commuting subsidies are distributed between different groups of workers. While it is mostly assumed that high-wage workers benefit more than proportionally from the subsidy due to higher marginal income tax rates and longer commuting distances (Bach, 2007), little is known about the specific distribution of benefits from commuting subsidies across wage groups.³ In addition, we lack an understanding of the spatial component of commuting subsidies, i.e., of the extent to which income is redistributed from cities to rural areas or vice versa. This effect is ex-ante ambiguous since workers living in the countryside commute longer distances while at the same time they earn less compared to workers living in urban areas (see Glaeser and Maré (2001), among others, on the existence of urban wage premia) and are hence subject to lower marginal tax rates. In the second part of the paper, we therefore shed light on the distribution of benefits from commuting subsidies between workers of different wage groups and between regions by degree of agglomeration.

A precise estimation of these distributional effects becomes possible through the availability of a large and novel data set which contains geo-referenced information on the exact place of work and place of residence of each worker. Drawing on this data and using GIS-software, we construct an accurate worker-specific measure of real commuting distances, which has not been available so far. Combining this measure with information on gross wages of about two million workers per year allows for determining changes in individual net wages as a result of the partial repeal of commuting subsidies.

We contribute to the literature in three major respects. First, we shed light on the

³In 2010, the German Green Party expressed their concern that commuting subsidies favor mainly higher income groups in an official inquiry to the Federal Government (“Kleine Anfrage an die Bundesregierung zur Verteilungswirkung der Entfernungspauschale”, Bundesministerium der Finanzen (2010)). In its response, the Government stated that information on the correlation between personal income and the size of individual tax breaks for commuting costs is not available.

question whether workers or employers effectively benefit from the substantial sum that is spent annually on commuting subsidies⁴ and how these benefits are distributed across different groups of workers. Second, we provide an estimate of the extent to which workers are compensated for commuting costs by their employers.⁵ The results from this estimate also provide a test of the urban efficiency wage models proposed by Zenou and Smith (1995) and Zenou (2006). Third, understanding the distributional effects of the reform allows to infer on the equity effects of the two major regimes of commuting subsidies prevailing in different countries. In countries like, e.g., Finland and Norway, commuting costs can be deducted without a lower bound on commuting distances while Sweden, Denmark, and Austria among others foresee tax deductions only for distances above a certain threshold (see Potter et al. (2006) and Borck and Wrede (2009) for an overview). The German case is unique inasmuch as both regimes were consecutively implemented within one country. We exploit this rare opportunity to consistently estimate the distributional effect of a paradigm shift in the design of commuting subsidies.

The results show that workers are only imperfectly compensated by their employers for the losses in net wages they incur as a result of the partial repeal of commuting subsidies. Overall, we find no evidence for gross wage adjustments for the full sample of workers. For workers mostly uncovered by collective wage agreements, however, the results suggest the existence of compensatory payments in the magnitude of eight percent of the net wage losses workers incur as a result of the reform. Overall, these results indicate that commuting costs are largely borne by workers. With respect to the distribution of benefits between worker groups, we find that commuting subsidies strongly favor high-earning workers and workers in rural areas. Consistently, these workers have largely carried the costs of the reform. This result is instructive because it shows that granting tax breaks only above a certain threshold of commuting distances lessens the regressive effect of commuting subsidies and yields a more equal distribution of benefits from commuting subsidies between regions.

The paper is structured as follows. In Section 4.2 we outline the design of commuting subsidies in Germany before and after the reform in greater detail. In Section 4.3 we summarize the data and provide descriptive statistics. In Section 4.4 we outline the difference-in-differences design we employ as our key identification approach to examine whether wage compensations are paid to workers affected by the reform and provide the results from the estimation. In Section 4.5 we shed light on the distribution of benefits

⁴In Germany, the sum of foregone tax revenues from tax breaks on commuting amounts to 4.5 billion Euro annually (Bundesministerium der Finanzen (2010)), which corresponds to 0.4% of overall public expenditures.

⁵In this regard, the paper is similar to Mulalic et al. (2013), who use firm relocation as a source of exogenous variation in commuting costs. The reform of commuting subsidies we draw on provides, however, not only a larger shock in terms of net wages, but also affects a much larger number of workers.

across wage groups and between workers in urban and rural regions, and discuss the distributional consequences of the two major paradigms of commuting subsidies prevailing in OECD countries. Section 4.6 concludes.

4.2 The Reform of Commuting Subsidies in Germany between 2004 and 2009

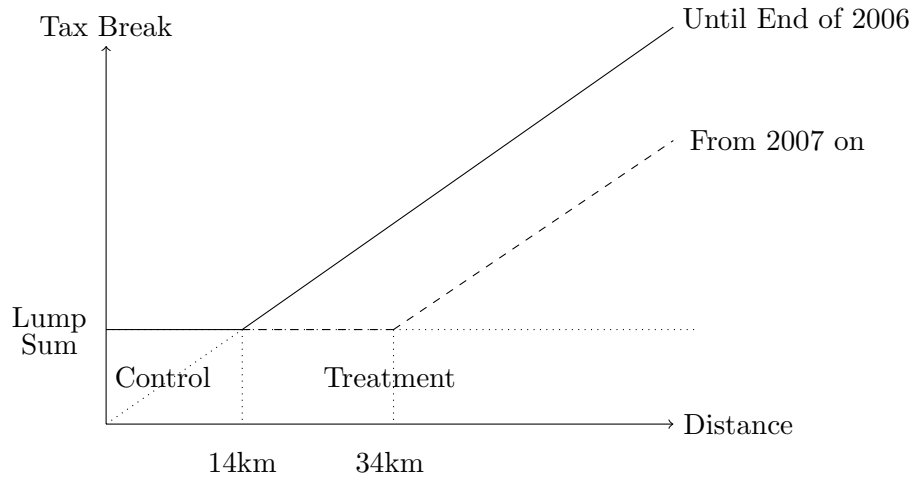
The reform of commuting subsidies we draw on in this paper was implemented between 2006 and 2007. Before 2007, commuting costs were legally considered as income-related expenses, i.e., as necessary costs incurred by workers for taking up and sustaining a specific employment. Analogous to the taxation of self-employed, where costs reduce taxable revenues, workers could offset a lump-sum of 920 Euro per year for income-related expenses against tax. If these expenses exceeded a total of 920 Euro, workers could alternatively deduct 0.30 Euro per kilometer of a one-way commute per working day from their taxable income. Tax authorities automatically apply the most favorable option for each worker.

Facing the urgent need to consolidate an increasing deficit in public budgets (Deutscher Bundestag, 2006), the German Parliament passed a reform of tax legislation on July 26th 2006, which stipulated a substantial reduction of foregone revenues from tax breaks on commuting costs. The new law, which came into effect on January 1st 2007, can be interpreted as a paradigm shift as it declared commuting to be privately caused and as such not part of income-related expenses. As a result, while the lump-sum regulation remained in effect, commuting costs exceeding an amount of 920 Euro were not deductible anymore. However, as completely abolishing the subsidy above this threshold was politically unfeasible, commuters traveling more than 20km per way were still granted a tax exemption of 0.30 Euro per kilometer from the 21st kilometer onwards. This was officially referred to as ‘hardship regulation’ (*Härtefallregelung*).

Figure 4.1 illustrates how the reform has altered the distribution of individual tax breaks as a function of commuting distance. Under the pre-reform regime represented by the solid line, individual tax savings from the per-kilometer deduction exceeded those from the lump-sum of 920 Euro from the 14th kilometer onwards.⁶ Hence, workers with commuting distances below 14km were assigned the lump-sum by tax authorities, while workers commuting 14km or more between their places of residence and of work were better off deducting 0.30 Euro per kilometer per work day.

⁶This value is calculated as $920/223 \cdot 0.30 = 13.75$, where 920 is the lump-sum, 223 the average number of contractual working days for a full-time employee per year, and 0.30 Euro the deductible amount per kilometer of a one-way commute per day. As the deduction is granted for full kilometers without rounding, the deductible amount under the per-kilometer regulation exceeds the lump-sum from the 14th kilometer onwards.

FIGURE 4.1: Policy Reform and Classification of Treatment and Control Group



By excluding the first 20km from tax deduction, the reform of commuting subsidies has effectively shifted this kink upwards to a daily one-way commuting distance of 34km. Workers commuting less than 14km per way were unaffected, as they claimed the lump-sum both before and after the reform. In contrast, all workers with commuting distances above this threshold experienced a reduction in commuting subsidies. As indicated by the dashed line, workers commuting between 14 and 34km were assigned the lump-sum after the reform rather than 0.30 Euro per kilometer. Workers commuting 34km or more would claim the per-kilometer deduction under both regimes, but in the later period were granted a deduction only from the 21st kilometer onwards.

We exploit this difference in the extent to which workers were affected by the reform to identify how the benefits from commuting subsidies are distributed between workers and firms. As explained in more detail in Section 4.4.1, we employ a difference-in-differences design, where we classify workers commuting 14km or more as the treatment group, and workers with commuting distances below 14km as the control group.

In December 2008, the Federal Constitutional Court declared the new regulation as unconstitutional. It argued that treating commuting costs as income-related expenses only for certain distances violates the general principle of equal treatment (*Allgemeiner Gleichbehandlungsgrundsatz*). After a period of controversial discussions on alternative ways to bridge the gap between political feasibility and conformity with superordinate law, the German Government repealed the reform in March 2009 and reinstituted the scheme prevailing before 2007.

4.3 Data and Descriptives

4.3.1 Data

For the analysis, we employ administrative data from the Federal Employment Agency which are provided by the Institute for Employment Research in the IEB (Integrated Employment Biographies). The IEB contains information on all employed persons subject to statutory social security contributions, as well as on all recipients of unemployment insurance or unemployment assistance. For these persons, information on demographic characteristics like education, age, gender, nationality, full-time vs. part-time employment, occupation, and wage, as well as on firm characteristics like establishment size and industry classification are provided. Important for our purpose, the data set can be extended upon special request by the exact geo-coordinates of each worker's place of living and place of work for the years 2007 to 2009 (see Scholz et al. (2012)).

We construct an annual panel of all full-time workers based on June 30th for each year. From this panel, we draw a 25% random sample for 2006, the year before the reform was implemented, and add all observations contained in the data for the years between 2004 to 2008. By excluding earlier and later years we account for the fact that the regime of commuting subsidies has changed in 2003 and in 2009. In addition, we restrict the sample in the following ways.

First, in order to avoid bias from selective sample attrition we include only individuals with a full set of employment observations. Doing so we address the concern that older workers who retire during the period of observation earn on average higher wages and commute smaller distances than young workers entering the labor market. Without this restriction, compensatory effects are likely to be underestimated due to changes in the composition of the treatment and the control group between the pre- and the post-reform period.

Second, we keep only workers with constant places of residence and work. This restriction is necessary because geo-coordinates are only available for the years 2007 to 2009, so we need to impute commuting distances for prior years.⁷ In addition, it accounts for the finding that workers affected by the reform have a higher propensity to reduce their commuting distance (Weiss (2009) and Boehm (2013)). If this option is predominantly chosen by workers who have no prospect of successfully negotiating for a wage compensation, their relocation would systematically alter the composition of the treatment and control group and, as a result, lead to an overestimation of gross wage adjustments. This condition also implies that workers do not change firms during the period of observation,

⁷We address the issue of missing geo-coordinates for the years 2004 to 2006 by keeping all workers who do not change their county of residence (NUTS III) between 2004 and 2008 in the sample. With respect to places of work, we keep workers with constant establishment ID and constant region of work during this period. The latter restriction also accounts for the (rare) occasion of establishment relocation.

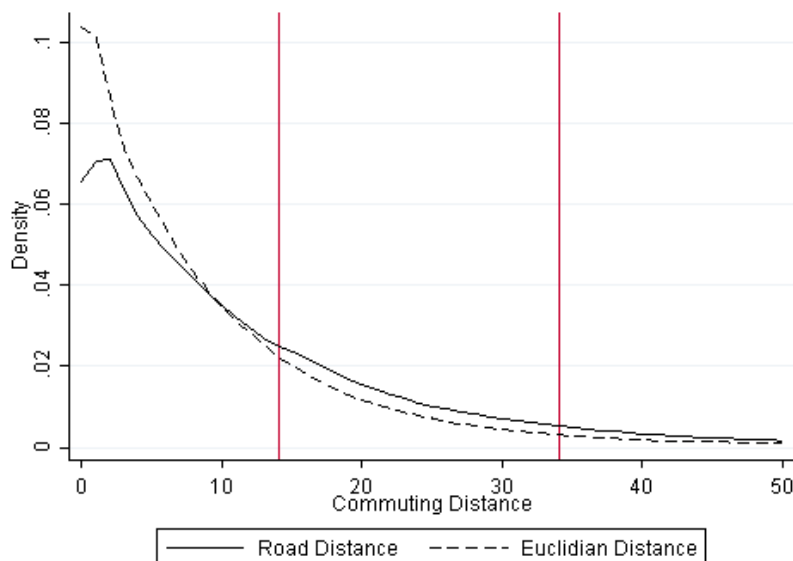
which rules out any selectivity of job changes between treatment and control group and, in particular, any confounding changes in wages from such selectivity.

Third, we exclude commuting distances that cannot realistically be covered on a daily basis. This is particularly relevant in the present context, as long-distance commuters often maintain a secondary residence in the region they work in. Since the geo-referenced data contain only information on primary places of residences, including these workers would lead to grossly overestimated commuting costs. We therefore exclude individuals with commuting distances exceeding 200km, which applies to 1.3% of full-time workers.⁸ Finally, we address the fact that wages are censored at the upper limit of social security contributions. In our sample, this applies to 5% of the observations. Imputing these wages is not feasible because this precludes any potential wage adjustment at the time of the reform, leading to an underestimation of wage effects. In order to avoid bias from measurement error, we delete observations with wages on or above this ceiling (see Reichert (2014)). All wages are deflated to the 2006 level.

The resulting data set is a balanced annual panel containing 9.99 million observations on about two million workers for the years 2004 to 2008. For each of these workers, we calculate both the Euclidean ('as the crow flies') and the shortest road distance between place of residence and place of work using GIS software and drawing on street layers provided by OpenStreetMap. The shortest road distance is stipulated by tax legislation as the basis for calculating the tax break for commuting costs. We use the Euclidean distance, which is independent from potentially inaccurate or incomplete maps, to detect mismeasurement in the road distances, which might arise from imprecision in the maps used in the GIS routine (mainly arising from private or unmarked roads in rural areas). Both measures show a correlation of 0.995 and in less than 0.04% of the observations the road distance is smaller than the Euclidian distance. We exclude these cases from the sample. Based on road distances, we assign each individual to either the treatment or the control group.

In order to examine the distribution of benefits from commuting subsidies across worker groups in Section 4.5, we need to calculate the size of individual tax savings from commuting subsidies under both regimes. We therefore draw on the approach proposed by Gunsellmann (2014), to which we add individual commuting distances. The procedure of deriving individual tax savings from information on gross wages and on commuting distances is described in more detail in the C.

FIGURE 4.2: Distribution of Commuting Distances



Note: Densities calculated based on Epanechnikov kernel; vertical lines indicate reform thresholds at commuting distances of 14 and 34 kilometers, respectively.

4.3.2 Descriptives

Figure 4.2 contains the distribution of commuting distances for both road and Euclidian distances. The average road distance (14.7km) exceeds the average Euclidian distance (11.0km) by 34%. The two curves intersect slightly below the threshold of 14km. This emphasizes the need to use true road distances instead of Euclidian distances, since workers affected by the reform would otherwise incorrectly be assigned to the control group. The fact that there is no indication for the existence of any kink or discontinuity in the distribution supports the notion that the thresholds at 14 and 34km do not coincide with any other salient points induced by other tax regulations, policies, or reforms.

Table 4.1 contains summary statistics of observable worker characteristics by treatment status. The treatment and control group contain 865,582 and 1,133,010 workers, respectively. With five observations per worker, the balanced panel encompasses 4.33 million observations on treated and 5.67 million observations on untreated workers. Treated workers are slightly younger, better qualified, and are employed in larger firms compared to workers in the control group. The effect of the reform becomes evident in the average amount that both groups can deduct before and after the reform. Workers in the control group offset the lump-sum of 920 Euro against taxes in both years. In contrast, workers in the treatment group deduct on average 3,412 Euro of commuting

⁸The results remain unchanged if we reduce this threshold to 100km.

TABLE 4.1: Worker Characteristics by Treatment Status

	Overall	Control	Treatment
No. Obs.	9.99 mio	5.67 mio (56.69%)	4.33 mio (43.31%)
15-24 years	2.14%	2.24%	2.02%
25-34 years	17.16%	16.88%	17.54%
35-54 years	68.36%	67.71%	69.22%
55-64 years	12.33%	13.18%	11.21%
Low Qualification	12.19%	13.66%	10.27%
Medium Qualification	72.43%	71.60%	73.52%
High Qualification	5.85%	5.11%	6.83%
Mean Firm Size	278	188	395
Mean Tax Deduction (Euro)			
2006	1,767	920	3,412
2007	1,197	920	1,734
Mean Tax Savings (Euro)			
2006	302	160	578
2007	213	161	316
Mean Net Wage (Euro)			
2006	19,374	18,557	20,959
2007	19,638	18,981	20,914
Mean Gross Wage (Euro)			
2006	32,632	31,407	34,236
2007	33,052	31,780	34,716

expenses before, and only 1,734 Euro after the reform.⁹ Consistently, with 160 and 161 Euro, average net tax savings from commuting subsidies remain constant for workers in the control group. For workers in the treatment group, tax savings fall from on average 578 Euro in 2006 to 316 Euro in 2007. As a result, while net wages rise by 2.3% for the control group, they fall by 0.2% for the treatment group. In contrast, with an increase of 1.4%, gross wages rise slightly more in the treatment group than in the control group (1.2%).

⁹The value for the post-reform period compares well to the amount of 1,603 Euro reported by the Federal Statistical Office as the average sum deducted in 2008 by all workers who do not claim the lump-sum when offsetting their commuting expenses against tax (Destatis (2012)). The slightly smaller value for the full sample is likely driven by part-time employed workers, who are excluded from our sample and who exhibit smaller commuting distances than full-time workers (Van Ommeren and Rietveld (2005)).

4.4 Commuting Subsidies and Gross Wage Compensation

4.4.1 Empirical Approach

We address the question whether workers are compensated by their employer for the net wage losses they effectively incur as a result of the reform of commuting subsidies. We apply a difference-in-differences (DID) approach where we exploit the fact that workers are affected differently by changes in the legal regulation of tax breaks for commuting costs. The treatment group consists of workers living away 14km or more from their workplace, who from January 1st 2007 onwards cannot offset the first 20km against tax anymore. The control group comprises all workers living closer than 14km from their workplace, who claim the lump-sum deduction before and after the reform. If employers compensate workers for losses in net wages resulting from the policy change, gross wages in the post-reform period should rise significantly more for workers in the treatment group compared to workers in the control group. We specify the following DID model

$$\log(grosswage)_{it} = \alpha + \lambda d_t + \delta(T \times d)_{it} + \mathbf{X}'_{it}\beta_1 + \mathbf{Z}'_{it}\beta_2 + \mu_i + \varepsilon_{it} \quad (4.1)$$

where T is a dummy variable which equals one if the commuting distance of worker i equals or exceeds 14 kilometers. d_t denotes pre- and post-reform periods and takes a value of one for the years 2007 and 2008.¹⁰ The matrices \mathbf{X}_{it} and \mathbf{Z}_{it} contain observable control variables on worker and firm-level, respectively. We control for unobserved heterogeneity between workers by means of individual fixed effects μ_i , which also capture time-invariant firm characteristics since workers in the sample by definition do not change firms. As a result, we effectively estimate the size of within-firm wage adjustment for workers of different treatment status. Note that μ_i also captures the effect of belonging to the treatment group because treatment status is constant over time as a result of commuting distances being fixed. Standard errors are clustered on the level of region of work in order to account for serial correlation (see Bertrand et al. (2004) and Cameron and Miller (2011)). The coefficient of interest is δ , which multiplied by 100 measures the average change of gross wages in percentage points for treated workers after the reform.

While the inclusion of fixed effects and other covariates accounts for systematically higher wages of workers in the treatment group, it does not control for differences in wage dynamic between both groups. Since such differential growth paths would pose a threat to the common trend assumption, we also estimate equation (4.1) with the

¹⁰Note that since in 2006 the panel date (June 30th) precedes the day the reform was passed in Parliament (July 26th), δ is unlikely to be biased by anticipation effects.

dependent variable in first differences:

$$\Delta \log(\text{grosswage})_{it} = \alpha + \lambda d_t + \delta(T \times d)_{it} + \mathbf{X}'_{it}\beta_1 + \mathbf{Z}'_{it}\beta_2 + \mu_i + \varepsilon_{it} \quad (4.2)$$

In the resulting equation (4.2), $\delta \times 100$ measures the average change in the growth rate of gross wages in percentage points for treated workers. We conduct placebo tests in order to examine whether the common trend assumption is satisfied in either specification. Before presenting the empirical results, we discuss two issues that might call into question whether gross wages can be realistically expected to adjust to changes in commuting subsidies.

First, one might doubt that wages adjust to reductions in commuting subsidies if predominantly negotiated by labor unions. Usually, collective wage agreements are closed for longer periods of time and do not take into account peculiar circumstances of subgroups of workers. This view has recently been challenged by Dustmann et al. (2014) and Card et al. (2013), who show that the influence of unions has declined decidedly in Germany since the 1990s and that, as a result, the importance of worker- and firm-specific pay premiums has increased substantially. In Section 4.4.2, we address this issue by estimating equation (4.2) separately for workers employed in industries with a low coverage rate of collective wage agreements.

Second, workers and firms might have expected the policy to be only of temporary nature and therefore have not engaged in wage negotiations in the first place. The history of the reform shows that over a period of nearly three years, i.e., between June 2006 and March 2009, there was no indication that the pre-reform regime would be reinstated. In January 2008, i.e., one and a half years after the reform was approved by the German Parliament, the Federal Fiscal Court regarded the new law as unconstitutional. This ruling was, however, not binding for the German Government and it took one more year for the Federal Constitutional Court to confirm the decision in December 2008. With another delay of three months, on March 19th 2009, the Parliament finally passed a law which reinstituted the legal status prevailing before 2007. During this later period, the future of commuting subsidies was unclear, with a full-scale abolition or a reduction to 0.10 Euro per kilometer being the most likely options discussed. Existing evidence on intra-firm wage policies suggests that a period of three years suffices for workers and firms to reach an agreement on how to share the burden of additional commuting costs. In fact, a large literature starting with Baker et al. (1994) has shown that the rising variation in individual wages is partly a result of wages reacting with increasing flexibility to changes in external conditions. In addition, Bingley and Lanot (2002) provide evidence that the costs of higher marginal tax rates are partially shifted from workers to employers. Similarly and directly related to the case of commuting subsidies, Mulalic et al. (2013) show that Danish firms compensate workers within a period of three years

after an exogenous shock on commuting costs.

4.4.2 Results

The upper panel in Table 4.2 contains results from estimating equations (4.1) and (4.2) for different samples of workers. The lower panel provides results from placebo tests, where we restrict the sample period to the years 2004 to 2006 and artificially move the date of the reform to January 1st 2006.

In column (1) we estimate equation (4.1) for the full sample. As indicated by the post-dummy, wages (deflated to the 2006 level) have risen on average by 0.72% for all workers in the post-reform period compared to the pre-reform years. The positive and significant estimate for δ shows that the policy change has led to an additional average rise in gross wages of 0.45% for workers affected by the reduction of commuting subsidies. The result from the placebo test in the lower panel indicates, however, that this is likely to be driven by different growth paths of wages of treated and untreated workers. Although the effect is 40% smaller in magnitude, a positive and significant wage effect remains for a period where no reform has taken place. In the remaining columns we therefore use wage growth rather than wage level as dependent variable.

For the full sample of workers in column (2), we find no evidence for a change in gross wage growth in response to the reform. As discussed in Section 4.4.1, this insignificance might be driven by workers whose wages are bound by collective wage agreements. We test this presumption in column (3). Based on information on industry coverage provided by the Federal Statistical Office (Destatis (2013b)), we estimate equation (4.2) only for workers employed in industries where less than half of the workforce is covered by collective wage agreements. For these workers, the results provide evidence for significant wage adjustments in response to the reform. The point estimate of 0.0008 suggests that wages of workers affected by reductions in commuting subsidies grow by additional 0.08 percentage points in the period after the reform. The size of these estimates is slightly smaller than the findings by Mulalic et al. (2013), who provide evidence for gross wage adjustments of about 0.15 percent for each additional kilometer after a firm relocation over a period of three years. Since the average change in commuting distances in their study is 1.2km, the average rise in gross wages amounts to 0.18 percentage points. Assuming a linear adjustment process over time, after a period of two years wages should have risen by 0.12 percentage points, which is comparable to our result for the two-year period from 2007 to 2008.

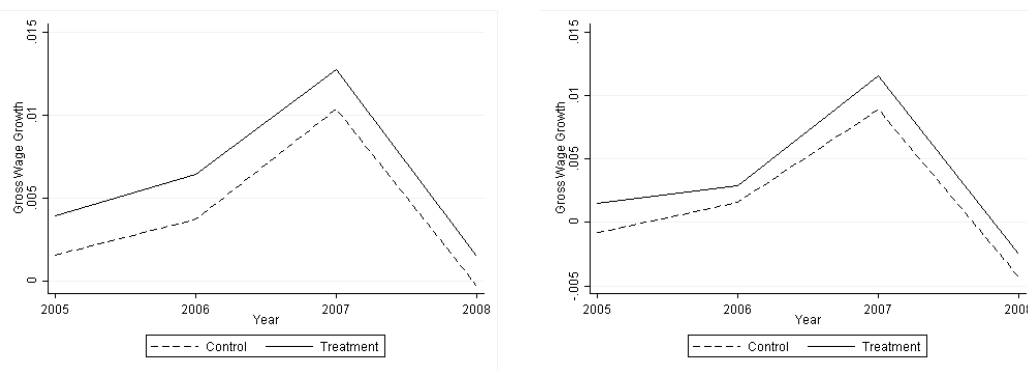
The lower panel shows the results from placebo tests. None of the results in columns (2) and (3) is statistically different from zero. In Figure 4.3 we additionally plot the average

TABLE 4.2: Gross Wage Adjustments as Response to the Reform of Commuting Subsidies

Sample	Full sample			Without collective wage agreements			LT
Sample Restriction							
Dependent Variable	$\log(wage)$ (1)	$\Delta \log(wage)$ (2)	$\Delta \log(wage)$ (3)	$\Delta \log(wage)$ (4)	$\Delta \log(wage)$ (5)	$\Delta \log(wage)$ (6)	$\Delta \log(wage)$ (7)
Post	.0072*** (.0005)	.0067*** (.0011)	.0073*** (.0004)	.0058*** (.0005)	.0056*** (.0005)	.0052*** (.0005)	.0058*** (.0005)
Treat×Post	.0045*** (.0004)	.0004 (.0004)	.0008** (.0002)		.0008*** (.0002)	.0009*** (.0002)	.0005*** (.0002)
Treat1×Post				.0008*** (.0002)			
Treat2×Post				.0007 (.0004)			
Treat×Post $placebo$.0027*** (.0005)	.0006 (.0008)	-.0005 (.0004)		-.0002 (.0003)	-.0002 (.0003)	-.0007 (.0004)
Treat1×Post $placebo$.0001 (.0004)			
Treat2×Post $placebo$				-.0011 (.0007)			
Mean Dep. Var.	10.33	.0052	.0027	.0028	.0028	.0021	.0028
Av. Gross Wage 06	32,632	32,632	29,971	29,971	29,960	26,235	29,971
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	9.99 mio	7.99 mio	4.76 mio	4.76 mio	4.10 mio	3.93 mio	4.76 mio

Clustered standard errors in parentheses, clustering on worker's region of work. * / ** / *** indicate significance at the 5%/1%/0.1% level, respectively. Columns (1) and (2) represent estimations with the full sample, Columns (3) to (7) contain only individuals working in industries with a coverage rate by collective wage agreements of less than 50%. Column (5) additionally excludes all workers who commute more than 87km, which is the legal maximum for costs from public transportation. In column (6) we exclude workers with wages in the upper quartile of the wage distribution. In column (7) a threshold of 9km is applied, assuming that workers claim only 60% of income-related expenses for commuting. Independent variables: Treat=1 for workers affected by the reform. Post=1 in 2007 and 2008, when the policy was in effect. Control variables encompass age, age², and firm size. In the placebo tests, observations cover only the years 2004 to 2006, with Post $placebo$ =1 for 2006.

FIGURE 4.3: Gross Wage Growth by Treatment Status



Note: The figure shows average growth rates of gross wages by treatment status for the full sample (left panel) and for workers employed in industries with a coverage rate by collective wage agreements of less than 50% (right panel).

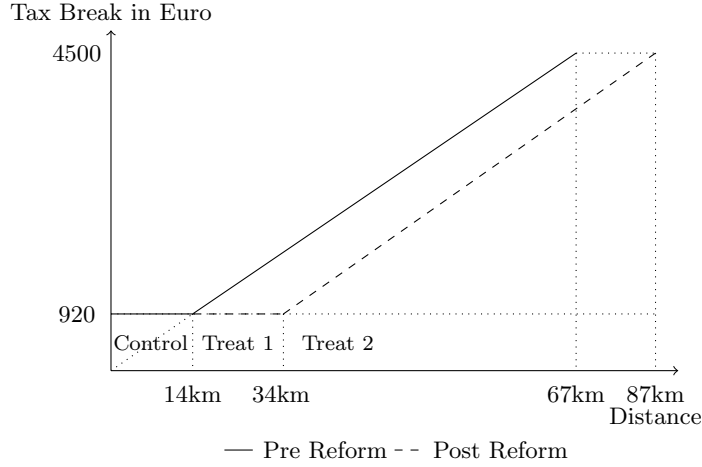
growth of gross wages per year separately by treatment and control group. For both samples, the two lines clearly exhibit a common trend. Both results refute the suspicion of systematic differences in growth rates between the treatment and control group and support the notion that the additional wage growth found in column (3) can indeed be attributed to wage compensation paid to workers who have incurred net wage losses as a result of the reform.

Understanding the magnitude of these wage compensations relative to the net wage losses allows for drawing inference on how commuting costs are distributed between workers and firms. Given average gross wages of 29,971 Euro for workers unbound by collective wage agreements, the results in column (3) imply an average rise in gross wages due to compensatory wage payments of 24 Euro. With average marginal tax rates of 19.3%, this yields a positive net wage adjustment of 19 Euro. The average size of net wage losses as a consequence of the reform amounts on average to 241 Euro per year. Taken together, these numbers suggest that workers are compensated for eight percent of the additional commuting costs they effectively incur as a result of the reform.

In the remaining columns of Table 4.2, we address a number of issues that pose a potential threat to a proper identification of compensatory wage payments. Throughout all specifications we use gross wage growth as dependent variable.

First, so far we have merged all workers negatively affected by the reform into one treatment group. As Figure 4.4 illustrates, the reform has, however, brought forth two groups of treated workers who are affected differently by the reform. The first group consists of workers commuting between 14 and 33km, who deduct the lump-sum after the reform instead of the per-kilometer amount they have claimed before. The second group comprises workers commuting 34km or more to work. In order to examine potential differences in wage compensations paid to either group, we estimate the size of

FIGURE 4.4: Illustration of Robustness Checks



Note: 920 Euro denotes the lump-sum deduction. 4500 Euro is the maximum amount deductible per year when using public transportation, which translates into an upper bound of 67 (87)km, above which workers cannot deduct commuting expenses in the pre- (post-)reform regime. The figure also illustrates the differentiation between two groups of treated workers, which we address in the first robustness check.

the treatment effect separately for each of the two groups. Equation (4.3) contains the augmented specification with two treatment groups:

$$\begin{aligned} \Delta \log(\text{grosswage})_{it} = & \alpha + \lambda d_t + \delta_1(T_1 \times d)_{it} + \delta_2(T_2 \times d)_{it} \\ & + \mathbf{X}'_{it}\beta_1 + \mathbf{Z}'_{it}\beta_2 + \mu_i + \varepsilon_{it} \end{aligned} \quad (4.3)$$

As shown in column (4), the treatment effect is positive and significant for the first group and does not change in size compared to using one treatment group. In contrast, while the coefficient for the second treatment group also remains constant in size, it is not significant. Given the larger standard errors with otherwise unchanged point estimates, this insignificance might be caused by a loss of statistical precision of the estimates. Since, however, the sample size is about the same for both treatment groups (1.7 million in each group), the estimate more likely indicates that workers commuting larger distances are not compensated financially, potentially because their loss in subsidies is smaller in relative terms compared to the first treatment group. Importantly, the results show that the compensatory wage effects we find are not driven by workers with ‘extreme’ commuting distances, but rather by workers with medium distances between home and work.

Second, tax legislation stipulates an upper limit of 4,500 Euro for the deduction of commuting costs incurred from the use of public transportation. In Germany, 13 percent of workers regularly commute to work by metro, bus, light rail, or train (Breiholz et al., 2005). These workers can offset commuting costs against tax only below an upper

threshold of 67km before and 87km after the reform.¹¹ As illustrated in Figure 4.4, this introduces another (shifting) kink into the distribution of tax breaks. Effectively, this means that workers commuting more than 87km by means of public transport (e.g., by using transregional or high-speed trains) have no incentive to negotiate for higher gross wages as they are de facto not affected by the reform. In order to test whether the existence of this upper ceiling affects our results, we apply a maximum commuting distance of 87km and drop all observations above this threshold. As shown in column (5), this restriction does not change the results.

Third, workers may be compensated by means of non-wage benefits like company cars or tickets for public transportation. Representative survey data from the German Socio-Economic Panel (DIW (2013)) suggest that five percent of workers receive benefits of a kind that is relevant in this context, like the provision of a company car or the partial coverage of travel expenses. In our wage data, these types of compensation are not observable as they are not subject to social security contributions. We therefore address this issue indirectly by excluding those workers with the highest propensity to be compensated by means of non-wage benefits. According to Shiftan et al. (2012) and Gutiérrez-i Puigarnau and van Ommeren (2011), personal income is the most defining characteristic for the use of company cars. In fact, two thirds of all company cars are provided to workers in the upper quintile of the wage distribution. We lower this threshold to the upper quartile and drop all workers with annual gross wages of more than 39,690 Euro (75th percentile) in 2006. The results are contained in column (6). The coefficient rises from 0.0008 to 0.0009, supporting the notion that additional wage payments are partially substituted by non-wage compensations.

Fourth, we address the concern that if workers offset other income-related expenses against tax, this effectively lowers the threshold of commuting distances above which workers claim the per-kilometer rather than the lump-sum option. With an average of 60 percent, commuting expenses make up by far the biggest share of all work-related expenses (Destatis, 2013b). The next most relevant items are in decreasing order expenditures for work equipment (6.7%), double household allowances (5.4%), additional meal allowance (4.9%), job-related travel expenses (4.5%), contributions to professional associations (2.3%), and costs for home office (1.6%). Each of these items is by itself of minor importance and cannot be systematically attributed to single worker groups. This inhibits excluding particular groups of workers with a high incidence of other types of income-related expenses from the sample. We can, however, exploit the fact that the share of commuting costs (60%) is constant across all wage groups contained in our sample (Destatis, 2013b) as this allows to lower the threshold between treatment and

¹¹The threshold of 67km for the pre-reform period is obtained from the formula for determining the individual deductible amount: $223 \cdot 0.30 \cdot \text{Distance} = 4,500$. The reform effectively shifts this threshold up to 87km since the first 20km are not deductible anymore.

control group to 9 kilometers and to re-estimate equation (4.2) with the two groups defined accordingly.¹² This approach, however, comes at the cost of a potential underestimation of compensatory wage effects because a number of untreated workers will now mistakenly be classified as treated. Given this caveat, the slightly lower but highly significant point estimate of 0.0005 obtained in column (7) provides a lower bound for the size of compensatory wage payments.

Another concern is that workers potentially overreport their commuting distance to tax authorities with the intent to reduce their taxable income. For a number of reasons, such overreporting is unlikely to be a problem in the present context. The literature on bunching finds evidence for a systematic manipulation of tax files only at large kinks in the tax schedule and only for the self-employed (Saez (2010), Bastani and Selin (2014)). Since marginal tax rates are not clustered in brackets in the German tax schedule (Doerrenberg et al. (forthcoming)) and because self-employed are not contained in our data set, the occurrence of overreporting at particular points in the wage distribution is unlikely. In addition, unlike the Austrian system, for which overreporting has been found (see Paetzold and Winner (2014)), the German regulation on commuting subsidies does not contain salient points which encourage manipulation (see Figure 4.1). Furthermore, the distribution of commuting distances calculated from our data is similar to self-reported distances contained in the German Socio-Economic Panel (Pfaff (2013)). Finally, since any overreporting of commuting distances effectively lowers the threshold between treatment and control group, we have indirectly controlled for this in the prior robustness check, where we have reduced the threshold above which the per-kilometer deduction can be claimed.

Finally, urban economic theory predicts changes in commuting costs to capitalize in housing prices (Borck and Wrede (2005, 2009)). This idea is not at odds with the empirical results obtained so far. It rather implies that wage losses as well as wage compensations are passed on to land owners. A causal analysis of how housing prices adjust to changes in individual net wages is beyond the scope of this paper. Since we are, however, concerned with the distributional effects of commuting subsidies, we briefly address the question whether there is descriptive evidence that the subsidy benefits land owners rather than workers. In order to test this presumption one would ideally examine whether housing prices decrease for workers negatively affected by the reform. Unfortunately, it is not possible to merge information on individual housing prices to the IEB data. However, we can calculate the average commuting distance for each region from the worker data and examine whether average regional housing prices change with commuting distances before and after the reform. As an alternative approach, we exploit

¹²Reducing the lump-sum of 920 Euros by 40 percent leads to a critical value of 552 Euro above which workers claim the per-kilometer deduction. Solving the formula $223 \cdot 0.30 \cdot \text{Distance} = 552$ for *Distance* yields a threshold of 9km for full-time workers.

the fact that in our sample the average commuting distance of workers living in cities (11.6 kilometers) is about 25 percent smaller than that of workers living in rural areas (15.6 kilometers). Since net wage losses are larger for the latter group (see Section 4.5), we should see housing prices rise more in cities than in rural areas in the years after the reform. We implement both approaches in the following DID model:

$$\Delta \log(\text{landprice})_{rt} = \gamma + \rho d_t + \theta(C \times d)_{rt} + \nu_r + \eta_{rt} \quad (4.4)$$

The dependent variable is the growth of average selling prices for ready-for-building land in region r in year t . As before, d_t denotes pre- and post-reform periods. Depending on the approach, C measures the average commuting distance of all workers living in region r or, alternatively, is a dummy variable indicating whether a county (NUTS III) is classified as urban (*Kreisfreie Stadt*) or rural (*Landkreis*). In 2006 and 2007, 295 rural regions existed, which contained about 65 percent of the population, alongside 107 cities. ν_r denotes region-fixed effects. Drawing on data provided by the Federal Statistical Office on average prices per square meter of sold land per county (Destatis (2013a)), we estimate equation (4.4) separately for both indicators. The results are contained in Table (4.3). As indicated by the insignificant interaction effects, for none of the specifications we find evidence for a shift in the growth path of regional land prices after the reform. Although this finding can be caused by the small scale of compensation effects or by the sluggish adjustment of land prices, it suggests that the redistributive effects of the reform are not passed on to land owners but are largely borne by workers.

In sum, the robustness checks confirm the finding of small but significant wage compensations paid to workers who are negatively affected by the reform and who can individually negotiate their wage. These workers are compensated on average for eight percent of the net wage loss they incur from the reduction of commuting subsidies. These findings suggest that the costs from a partial repeal of commuting subsidies have mainly been borne by workers commuting 14km or more to work, in particular if they are covered by collective wage agreements. More generally, the results can be taken as evidence that workers are only imperfectly compensated for commuting costs by their employers.

4.5 The Distribution of Tax Savings across Worker Groups

If commuting subsidies are largely inspired by equity considerations it is of interest to examine how benefits are distributed between different worker groups. We now address this question by calculating the amount of tax savings, i.e., of individual net wage effects

TABLE 4.3: Capitalization of Commuting Subsidies in Land Prices

Dependent Variable	$\Delta \log(\text{Price Sold Land})$			
	(1)	(2)	(3)	(4)
Average Comm. Distance	.003 (.0017)			
City			-.014 (.0113)	
Post	.031 (.0359)	.030 (.0373)	-.048 (.0139)	-.054*** (.0146)
Av. Comm. Dist.*Post	-.004 (.0029)	-.004 (.0031)		
City*Post			.005 (.0276)	.011 (.0281)
Region Fixed Effects	No	Yes	No	Yes
Mean Dep. Var.			.0122	
Years			1996 - 2008	
N			4,637	

Standard errors in parentheses. */**/** indicate significance at the 5%/1%/0.1% level, respectively. Independent variables: City=1 for counties classified as urban (*Kreisfreie Stadt*). Average Commuting Distance is the mean commuting distance of all workers living within a county. Post=1 in 2007 and 2008, when the policy was in effect.

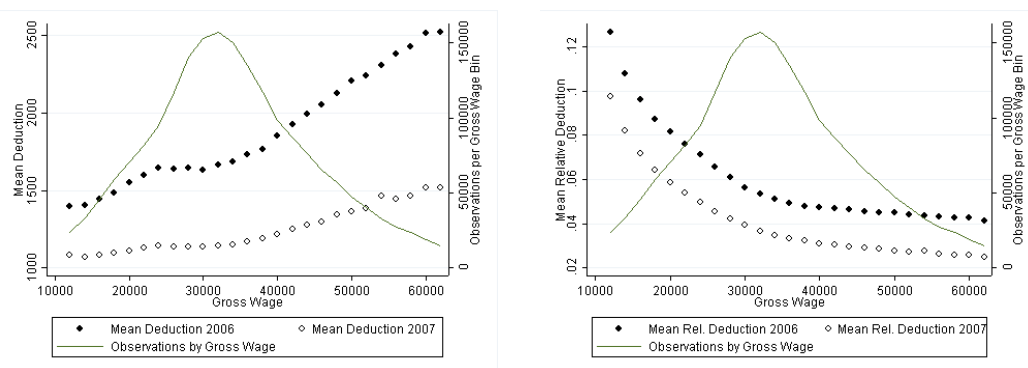
from the tax break, for each worker before and after the reform. We then examine the distribution of these tax savings between workers along two dimensions.

First, we consider to which extent commuting subsidies are regressive in nature by effectively attenuating the progression of marginal tax rates (Biewen and Juhasz (2012)). While it is ex ante likely that commuting subsidies benefit in particular workers with high wages as these are subject to higher marginal tax rates and on average commute longer distances, the specific distribution of tax savings between different income groups has so far remained unclear.

Second, commuting subsidies might exhibit a spatial component if they benefit workers in different regions to a different extent. A natural presumption would be that workers in rural areas incur larger net wage effects from tax breaks on commuting because they commute on average longer distances. This effect might, however, be counteracted by lower wages and correspondingly lower marginal tax rates in rural areas. Given the predominantly rural structure and comparatively low wages in East Germany, it is of particular interest with respect to the German case to examine whether workers in this part of the country benefit more or less than proportional from commuting subsidies.

We examine the income and the spatial dimension of tax savings from commuting subsidies in turn. Methodologically, we first calculate the average size of deductible amounts and of tax savings by worker group before and after the reform (we describe the exact procedure in Appendix C). We plot both against gross wages and against regional density in order to shed light on how their respective distribution has changed with the

FIGURE 4.5: Tax Deductions Before and After the Reform, by Gross Wage



Note: The figure shows average absolute deductions (left panel) and deductions relative to gross wage (right panel) before and after the reform in gross wage bins of 2,000 Euro in the range from 12,000 to 62,000 Euro. The kernel density shows the distribution of workers by gross wage.

reform. Regarding the spatial dimension, we then compute Theil indices (defined in Section 4.5.2) in order to disentangle whether changes in the distribution of benefits result from changes within or between region types. We use the same data as described in 4.3.1.

4.5.1 Distribution between Wage Groups

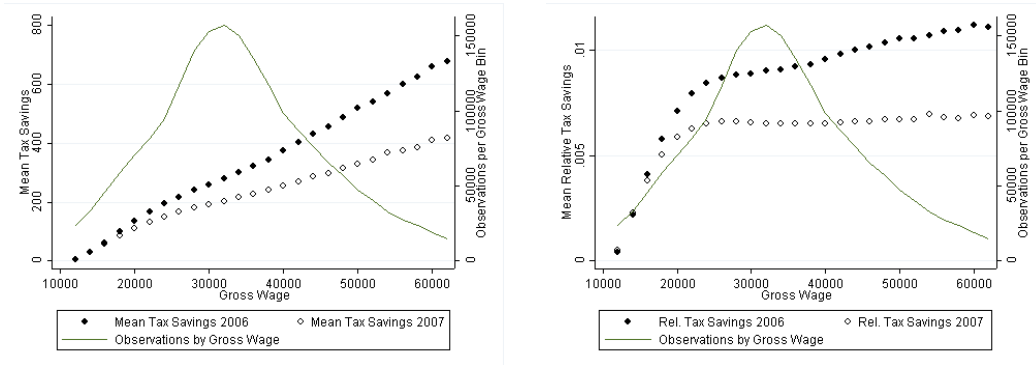
Figure 4.5 contains the distribution of average deductible amounts by gross wage groups in gross wage bins from 12,000 to 62,000 Euro in steps of 2,000 Euro before and after the reform.¹³ The kernel density of workers relates the distribution of deductible amounts to the number of workers affected.

The left panel shows the distribution of deductible amounts in absolute values, which increases with gross wage for both years. Before the reform, the sum of commuting costs that workers could offset against tax rises from an average of 1,400 Euro for workers earning 12,000 Euro per year to 2,500 Euro for workers in the 62,000 Euro bracket. Since the size of the deductible amount is independent of marginal tax rates, this distribution mirrors the fact that average commuting distances rise with gross wage. After the reform, the average deductible amount rises only moderately from 1,000 to 1,500 Euro along the distribution. With an average difference of 300 Euro for workers at the lower end of the distribution and of 1,000 Euro for workers at the upper end, the size of the loss increases in absolute values with gross wage.

The right panel shows the average deductible amount relative to gross wage within each

¹³The upper bound is the maximum threshold for social security contributions above which wages are censored in the data. The lower bound results from the fact that income taxation applies to all taxable incomes exceeding a minimum of 7,664 Euro. For workers with annual wages of 12,000 Euro and below, income taxes are effectively zero because numerous deduction possibilities reduce taxable incomes to below this threshold.

FIGURE 4.6: Tax Savings Before and After the Reform, by Gross Wage



Note: The figure shows average absolute (left panel) and relative (right panel) tax savings before and after the reform in gross-wage bins of 2,000 Euro in the range of gross-wages from 12,000 to 62,000 Euro. The kernel density indicates the distribution of workers by gross wage.

bin, calculated as $\frac{1}{n} \sum_i^n \frac{\text{deductible amount}_i}{\text{grosswage}_i}$ for the n individuals in each wage category. In contrast to the absolute values shown in the left panel, the relative importance of the deduction is largest for low-wage workers. In fact, workers earning 12,000 Euro can reduce their taxable income by 13% before the reform, while the corresponding value for the highest wage category is only 4.1%. The reform yields a parallel downward shift of the curve, leading to a reduction of deductible amounts between two and three percentage points for all worker groups.

In the left panel of Figure 4.6, we plot tax savings from tax breaks on commuting costs in absolute values against gross wage. Over the full range of gross wages, tax savings increase monotonically from zero to about 700 Euro per year. The right panel displays tax savings as a share of gross wage. It turns out that relative to their gross wage, lower wage groups profit least from commuting subsidies. Tax savings rise steeply up to a gross wage level of about 23,000 Euro and become flat thereafter at a share between 0.9 and 1.1% of gross income. With the reform, tax savings remain virtually unchanged for workers earning less than 20,000 Euro annually. In contrast, the decrease for higher wage groups is more pronounced. This is due to the combined fact that workers with higher gross wages commute longer distances and are subject to higher marginal tax rates due to progressive taxation. As a result, with a reduction from 1.1% to 0.6% of gross wage, the net wage effect of commuting subsidies for these wage groups has about halved.

Overall, the analysis shows that the regime of commuting subsidies prevailing before the reform was unambiguously regressive in nature inasmuch as it has reduced the tax burden predominantly for higher wage groups. The introduction of a lower bound on commuting distances combined with the option to deduct a lump-sum has led to a more equal distribution of tax savings by disadvantaging higher incomes while leaving lower

incomes largely unaffected.

4.5.2 Distribution between Regions

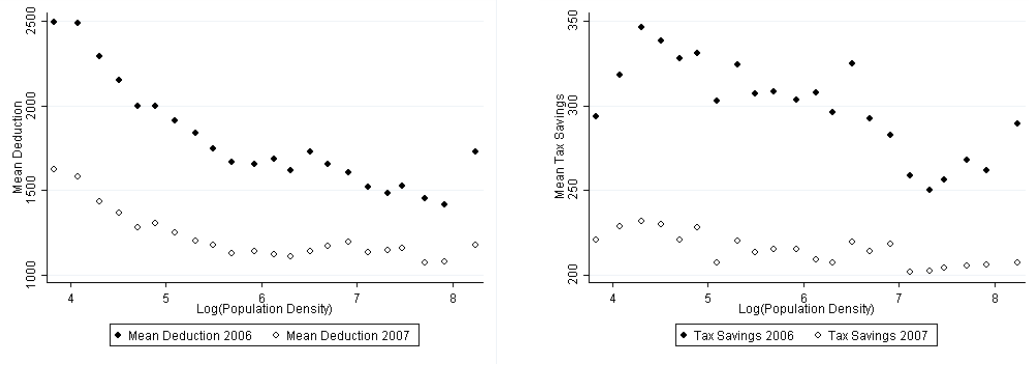
From a spatial perspective, it is of interest to see whether regional differences exist with regard to the distribution of net wage benefits from commuting subsidies and how the reform has affected this distribution. Specific to the German case, this is particularly relevant with respect to disparities between East and West Germany. Wage levels in the former East are still substantially lower than in the West, unemployment is higher and commuting distances are larger due to the rural nature of most areas. We therefore examine whether urban and rural regions, as well East and West Germany differ with respect to the distribution of tax savings before and after the reform.

In Figure 4.7, we plot the distribution of deductible amounts and of tax savings against logged regional population density in bins of 20 log points. Density is measured as residential population per square kilometer within each of the 403 NUTS III regions in Germany. Over the whole range of population density, the average deductible amount falls from 2,500 Euro in counties with a population density of 45 workers per square kilometer to 1,450 Euro in counties with a density of 3,846 workers per km^2 , reflecting the fact that commuting distances decrease with the degree of urbanization. The outlier on the rightmost extreme of the distribution represents Berlin. The average deductible amount is higher for Berlin compared to other core cities because the substantial spread of the city entails larger commuting distances. With the reform, deductible amounts decline most strongly for workers in rural areas, where losses on average amount to 900 Euro compared to 330 Euro in urban areas (excluding Berlin).

The right panel contains the corresponding distribution for tax savings. The negative slope is less pronounced compared to deductible amounts because larger commuting distances in rural areas are countervailed by higher gross wages and correspondingly higher marginal tax rates in urban areas. While tax savings were declining modestly with population density in the middle part of the distribution, the reform has virtually equalized the distribution of tax savings across regions. While tax savings ranged between 250 and 350 Euro in 2006, they turn out largely constant at around 220 Euro in 2007.

Theoretically, the observed reduction in inequality can result from changes in the distribution of tax savings within or between region types. We address this issue by means of the Theil index. As a key advantage, the index can be decomposed into K subgroups when mutually exclusive and exhaustive subgroups are considered (see, e.g., Shorrocks (1980)) and Morduch and Sicular (2002)). The Theil index and its decomposition are

FIGURE 4.7: Tax Deductions and Tax Savings Before and After the Reform, by Regional Density



Note: The figure shows average deductible amounts (left panel) and tax savings (right panel) before and after the reform by population density in bins of 20 log points for population densities within German (NUTS III) regions.

defined as

$$T = \underbrace{\frac{1}{n} \sum_i \frac{y_i}{\bar{y}} \ln \left(\frac{y_i}{\bar{y}} \right)}_{\text{overall}} = \underbrace{\sum_k \left(\frac{Y_k}{Y} \right) T_k}_{\text{within}} + \underbrace{\sum_k \left(\frac{Y_k}{Y} \right) \ln \left(\frac{\bar{y}_k}{\bar{y}} \right)}_{\text{between}} \quad (4.5)$$

where y_i denotes tax savings for individual i and \bar{y} the mean of y for all n individuals. The index ranges between 0 and ∞ , where a value of 0 indicates an equal distribution. The right hand side of equation (4.5) decomposes the index into a “within”- and a “between”-component. T_k represents the Theil index for subgroup k , i.e., for urban and rural workers or for workers in East or West Germany. Y and Y_k denote the sum of tax savings in the total population and within subgroup k , respectively.

For the decomposition to be meaningful, we need to classify regions into rural and urban regions. As in Section 4.4.2, we employ the categorization of German NUTS III regions into cities (*Kreisfreie Städte*) and counties (*Landkreise*).

The first row in Table 4.4 provides the Theil index for the distribution of tax savings in Germany separately by pre- and post-reform period. With a reduction of inequality by 30 percent, the impact of the reform has been substantial. The two lower panels shed light on how this overall decrease can be attributed to changes in the distribution of tax savings within and between rural and urban areas on the one hand, and within and between East and West Germany on the other hand.

The first two rows in the second panel contain the Theil indices separately for urban and rural areas. The distribution of tax savings is less equal in rural regions compared to urban regions and has decreased less with the reform compared to urban areas. When decomposing overall inequality into its within- and between-components, it turns out that inequality is nearly completely driven by the within-component. The between-component only adds about one percent to overall inequality, indicating that there are

TABLE 4.4: Decomposition of Theil Index of Tax Savings, by Region Type

	2004-2006	2007-2008	Δ
All Germany	0.3627	0.2572	-0.106
Urban only	0.3261	0.2068	-0.119
Rural only	0.3672	0.2736	-0.094
<i>Decomposition</i>			
Within	0.3577	0.2561	-0.102
Between	0.0050	0.0007	-0.004
Percentage of w/i	0.99	0.99	
East only	0.4058	0.3237	-0.082
West only	0.3512	0.2427	-0.109
<i>Decomposition</i>			
Within	0.3589	0.2149	-0.144
Between	0.0038	0.0023	-0.002
Percentage of w/i	0.99	0.99	

hardly any systematic differences between both region types. The reform has no effect on this pattern. A decomposition by East and West Germany in the third block yields largely the same picture. Tax savings are less equally distributed in the (mostly rural) regions in East Germany as compared to the West and this inequality has decreased less with the reform. Again, the between-component does not play a role regarding an unequal distribution of tax savings between workers.

Overall, the results indicate that the introduction of a lower bound on commuting distances combined with the option to deduct a lump-sum has led to a more equal distribution of tax savings with respect to the two dimensions considered. Regarding the distribution among wage groups, the new regime has equalized the distribution of tax savings relative to gross wages for workers with annual gross wages above 20,000 Euro, while leaving tax savings of lower wage groups unaffected. As such, the reform has reduced the regressive tax effect of commuting subsidies. Similarly, the new regime has led to a decrease of inequality in the distribution of tax savings across region types. The results from the Theil index suggest that the reform has reduced inequality within urban and rural regions, as well as within East and West Germany by about 30%. From a political view point, this latter point is particularly interesting because differences in population densities between East and West as well as between rural and urban areas have often been pointed out as reasons for the design of commuting subsidies before the reform (see, e.g., Bundesministerium der Finanzen (2010)).

4.6 Discussion

In this paper we examine the distributional effects of commuting subsidies along three dimensions. Employing a difference-in-differences approach, we first estimate whether workers are compensated by their employers for the net wage losses they incur as a result of large-scale reductions in commuting subsidies. The results show no indication of gross wage adjustments for the full sample of workers. Only for workers largely uncovered by collective wage agreements we find evidence for wage compensations in the magnitude of 8% of the net wage losses that workers have incurred from the partial repeal of commuting subsidies. These results support the notion that the bulk of commuting costs are borne by workers rather than firms.

In the second part of the paper, we analyze whether commuting subsidies are progressive or regressive in nature and how tax savings are distributed between region types. The results show that the benefits from commuting subsidies accrue more than proportionally to workers with higher wages. The introduction of a lower bound on commuting distances has reduced the size of these net wage benefits mainly for workers with annual wages above 20,000 Euro annually while leaving workers in lower wage groups unaffected. Regarding the distribution of tax savings from commuting subsidies between regions, we find that workers in rural regions benefit more than proportionally in terms of their net wages. The reform has largely equalized the distribution of tax savings between urban and rural areas and between East and West Germany.

Beyond their significance for the German case, these results are instructive for the design of commuting subsidies in general because they allow to compare the distributional effect of two regimes within an otherwise stable macroeconomic environment. Overall, the introduction of a lower bound for commuting distances, as it is currently practiced in Sweden, Denmark, and Austria, has the potential to substantially reduce the amount of foregone tax revenues from tax breaks on commuting costs while at the same time yielding a more equitable distribution of tax saving between wage groups and across regions. Interestingly, despite these effects no new reform attempt has been made in Germany after the ruling of the Federal Constitutional Court in 2008.

The recent availability of geo-referenced data provides ample opportunities for more extensive research in this area. An important aspect not covered sufficiently in the empirical literature relates to modal choice and, hence, to the environmental dimension of commuting subsidies. Specifically, whether commuting subsidies should provide incentives to use public instead of private transportation crucially depends on the worker-specific availability of public transport and on the demand elasticities. Combining large-scale information on public transportation schedules and on travel times with precise geographic information on worker location is well suited to shed light on potential efficiency as well as equity effects from transportation-specific financial incentives.

Chapter 5

Conclusion

My thesis discusses several spatial aspects of the labor market and thereby analyzes the interplay of individuals' location and their labor market outcomes.

I examine the relationship of residential segregation and wages in chapter 2 and neighborhood referral effects in chapter 3. In the light of the current European and especially German refugee crisis¹, questions concerning urban policies and the refugees' integration into the labor market are more prevalent than ever. Should segregation in cities be tolerated or even encouraged such that networks can emerge? Should new migrants be integrated and "forced" into predominantly German neighborhoods? Although chapter 2 and 3 emphasize the importance of proximity of "similar" neighbors in finding a job, these results are based on comparatively moderate numbers of migrants and especially migrant concentration. Häußermann et al. (2008) outline the historic development of German segregation which used to be characterized by a policy of assignment (controlled by specific quarters of accommodation for guest-workers) and discrimination. Nowadays especially middle class migrants can choose their residential location more freely. As now the neighborhood composition is mostly regulated by the market, this again leads to a reinforcement of impoverishment in certain areas. Everyone who can afford to move away will do so.

In Germany's current situation, the stream of incoming refugees is a challenge to all urban development and local authorities. Especially new migrants have an incentive to settle close to their fellow countrymen, because of language and cultural barriers. If those groups are mostly unqualified (or their qualification is not acknowledged), this can have serious consequences for urban landscapes. Both segregation - the topic of chapter 2 - and residential based networks - the topic of chapter 3 - are therefore more relevant than ever in the German political debate. Although chapter 2 suffers from specification

¹In 2015, Germany received almost 500,000 applications for asylum (which does not include all the unprocessed applications). This is equivalent to an increase of 235% compared to 2014. (BAMF, 2016)

and data problems, it highlights an important political issue. Chapter 3 addresses some of the most important shortcomings of the prior chapter, namely sorting and the specification of residential neighborhoods. Even if we find robust evidence for the existence of neighborhood effects it is unclear which policy would be appropriate.

Prominent examples of policies targeting this interdependence are relocation programs such as the Gartreux in Chicago in the 1970s or the Moving to Opportunity (MTO) program, which was conducted in Baltimore, Boston, Chicago, Los Angeles and New York in the 1990s (see e.g. Ludwig et al. (2008), Ludwig et al. (2013)). Low-income families with children who lived in public housing projects could voluntarily apply to participate in the program and were then randomly assigned to one of three groups: The first group received housing vouchers for low-poverty neighborhoods together with counselling for housing, the second group only received housing vouchers (also for neighborhoods with low poverty levels). The third group did not receive anything (serving as a control group). Albeit the extent of the program, the economic improvements of participants were lower than expected. There were no significant effects for economic or health improvements for adults, and only minor health improvements for youths four to seven years after the intervention (Goering and Feins, 2003). Long-run effects show that even though there are neither educational, employment or income effects, the policy positively affected people's health both for young and adult participants (Sanbonmatsu et al., 2011). Pettit and McLanahan (2003) qualitatively analyze the effectiveness of the program. They survey MTO participants' social connections in their old and new neighborhoods in Los Angeles. They find a higher degree of isolation especially for adult participants in low-poverty neighborhoods. This qualitative evidence illustrates the importance of social networks within neighborhoods, which emphasizes how external neighborhood effects can be undermined when those relocated choose their own peers. As the difference between the new and the old environment was intended to be substantial, social isolation and retaining to old (and presumably less valuable) contacts were the consequence. Information flows and local networks - as in chapter 3 - may therefore not be accessible for those who moved. Over all, empirical findings suggest that policies directed to improve economic opportunities in impoverished areas are more likely to be effective than mobility policies (Ross and Zenou, 2008). So targeting job training or other active labor market policies directly to neighborhoods with low income or high unemployment rates may be the better policy than to relocate impoverished families into better neighborhoods. In this way, the social networks stay intact such that new job information can be distributed most efficiently, this may be beneficial for an even higher proportion as those actually targeted by the policy.

All in all, the results of urban and relocation policies to improve economic opportunities by relocation or reorganizing the housing environment seem to have failed. The evidence from network studies and public housing experiments suggests, that people benefit the

most from mixed income communities.

To target disadvantaged areas with e.g. labor market policies, it is important to quantify the effect of residential location on personal outcomes as accurately as possible. Although chapter 3 overcomes some of the problems from chapter 2, there are still some critiques left. The main problem in chapter 3 is the lack of information on people's social interaction. Our measure of a network effect is only a proxy which has to be interpreted with caution. Topa (2011) suggests that residential neighborhoods contain the natural requirements for social interactions. They minimize costs of interaction, as they constitute of a physically proximate space. Moreover, they usually contain public places and potential meeting points like public schools, sports clubs, churches etc. where social interaction predominantly takes place. Nevertheless, as our data set consists of registry data, there is no information on actual interactions. As the data has many advantages (like its size, accuracy and scope of reliable information, described in section 3.3) I believe the approximation is a drawback that has to be accepted in order to measure local spillovers.

We make a great effort to address all potential confounders, but we cannot completely rule out all potential spurious correlation. Missing household information is one example of another data issue which can lead to spurious correlation. Especially small and medium size firms in Germany are largely dominated by family owned businesses. Although we exclude females in our regressions estimating firm size effects and see no differences here, it could still be the case that e.g. fathers and sons work disproportionately often together in the same establishment (e.g. family businesses of craftsmen or retail etc.), which would not be excluded in this particular robustness check. When fathers and sons live in the same neighborhood and work in the same family business, the likelihood of working together when living together is increased although there is no connection to informal job markets whatsoever.

In chapter 4, I analyze distributional effects of commuting subsidies. On the one hand, we test to what extent employers compensate workers in the absence of governmental commuting subsidies. We thereby analyze the distributional effects between workers and firms. On the other hand, we investigate the distributional effects of the policy change between worker groups as well as rural and urban workers.

We use a unique reform on the tax breaks on commuting in Germany in combination with a large geo-coded registry data set. Using the partial abolishment of commuting subsidies in Germany in 2007, where only commuting costs from 20km onwards were deductible, we employ a difference-in-differences strategy to estimate gross wage compensations for those affected by the policy. We find that in the full data set, workers experience no changes in gross wages as a reaction to the policy and small but significant and robust effects for workers largely uncovered by collective wage agreements. The latter group is compensated by 8% of their net wage losses. The largest part of tax burdens

in consequence of the reduction in subsidies is hence carried by workers. In the second part of the paper, we analyze how the benefits of commuting subsidies are distributed between groups of workers in the two subsequent regimes. It turns out that commuting subsidies prevailing before 2007 (which are the same as today) are clearly regressive in nature. The reform of 2007 which introduced a lower bound to the deductibility of commuting costs then led to a more equal distribution of tax savings across gross wages. In addition, the lower bound also inherently eliminates the differential benefits between urban and rural areas as well as between East and West. Further, and although beyond the scope of this particular research, subsidizing commuting from the first kilometer onwards has negative effects on environmental pollution (see also Bach (2007)). International comparison further affirms the interpretation that commuting subsidies should be capped at a maximum length and also encourage people to use public transportation instead of own cars². Although we do not observe changes in residential location as a reaction to the cutback of commuting subsidies (which is both due to data restrictions and the limited time the altered policy was in effect), the current rule has the unintended side-effect of urban sprawl.³ Instead, decision makers should use this means of influencing people's spatial distribution among space, which would benefit environmental and infrastructural goals. Moreover, people could benefit from social interactions and spillovers as mentioned in chapters 2 and 3.

In summary, I provide the following research contributions: First, I show that the correlation of migrants' individual wages and their concentration in residential neighborhoods is non-negative. Second, I provide credible evidence that there exist informal labor market networks based on residential locations, which are especially important for low qualified and minority workers. Third, I show that German workers are only compensated to a small extent in the absence of commuting subsidies. Additionally, designing commuting subsidies as a lump-sum deduction together with a lower bound on per-kilometer deduction leads to a more equal distribution of tax savings as opposed to without a lower bound.

²Tax legislation foresees an upper limit of 4,500 Euro if people commute with public transportation whereas there is no limit if people use their own or company cars.

³As Brueckner (2005) shows, commuting subsidies in general drive urban sprawl.

Appendix A

Appendix Residential Segregation of Immigrants in Germany

District	German	Foreigner
Hamburg city	939	130
	87.84%	12.16%
HH Altona	1,005	106
	90.46%	9.54%
HH Bergdorf	536	24
	95.91%	4.09%
HH Nord	1,535	91
	94.40%	5.60%
HH Wandsbek	1,679	92
	94.81%	5.19%
HH Eimsbüttel	1,182	87
	93.14%	6.86%
HH Harburg	789	104

TABLE A.1: Classification of Foreigner Types

Foreigner type 1 (F1)	Foreigner type 2 (F2)
Switzerland, France, Austria, Netherlands, Luxembourg, Belgium, GB, Denmark, Sweden, Finland, USA, Canada, Australia	Turkey, Italy, Greece, Poland, Croatia, Portugal, Spain, Russia, Rumania, Czech Republic, Ukraine, Hungary, Albania, Bulgaria, Slovenia, Estonia, Latvia, Lithuania, Rest of Europe, Ethiopia, Ghana, Morocco, Tunisia, Rest of Africa, Rest of America, India, Iraq, Iran, Thailand, Rest of Asia

	88.35%	11.65%
Frankfurt/ M.	2,654	600
	81.56%	18.44%
FfM Bad Homburg	1,140	103
	91.71%	8.29%
FfM Bad Vilbel	335	29
	92.03%	7.97%
FfM Höchst	1,561	249
	82.24%	13.76%
Langen	492	43
	91.96%	8.94%
Stuttgart City	912	247
	78.69%	21.31%
Böblingen	1,183	206
	85.16%	14.84%
Herrenberg	480	51
	90.21%	9.79%
Leonberg	527	74
	87.69%	12.31%
Stuttgart-Vaihingen	686	92
	88.17%	11.83%
Stuttgart-Feuerbach	459	148
	75.26%	24.74%
Bad Cannstatt	626	181
	77.57%	24.38%
Munich City	7,929	1,562
	83.54%	16.46%
Dachau	883	68
	92.54%	7.36%
Ebersberg	727	55
	92.97%	7.03%
Fürstentfeldbruck	1,157	84
	93.23%	6.77%
Starnberg	591	59
	90.29%	9.71%
Southern Berlin	857	85
	90.98%	9.02%
Treptow-Köpenik	1,04x	<20
	>98%	<2%

Steglitz-Zehlendorf	820	100
	89.13%	10.07%
Tempelhof-Schöneberg	1,121	232
	82.83%	17.17%
Northern Berlin	1,073	97
	91.71%	8.29%
Pankow	1,702	38
	91.71%	2.18%
Spandau	842	35
	95.93%	4.07%
Berlin Mitte	882	82
	91.49%	8.51%
Müllerstrasse	1,059	122
	89.98%	10.02%
Lichtenberg	1,380	28
	98.01%	1.99%
Marzahn-Hellersdorf	>1,530	120
	>99%	<1%
Reinickendorf	805	50
	94.15%	5.85%
Overall	43,337	5,163
	89.35%	10.65%

TABLE A.2: Foreigners' Nationalities conducted from BAP, wave 40

Nationality	number	percentage
Turkey	921	17.84
Italy	500	9.68
Form. Yugoslavia	372	7.21
Greece	288	5.58
France	133	2.58
Poland	160	3.10
Austria	254	4.92
Croatia	415	8.04
Portugal	116	2.25
Spain	95	1.84
Netherlands/ Luxembourg	53	1.03
Russia	98	1.90
Bosnia-Herzegovina	201	3.90
Great Britain/ Ireland	91	1.76
Czech Republic/ Slovakia	52	1.01
Ukraine	63	1.22
Macedonia	60	1.16
USA/ Canada	87	1.69
Afghanistan	85	1.64
Iran	117	2.27
Rest	1002	19.41
Only countries with more than 50 individuals living in Germany are listed in this table.		

Appendix B

Appendix Job Referrals Based on Residential Location Networks

TABLE B.2: Estimation of Heterogenous Referral Effects, Full Output

Variable	Refferal to neighborhood ($a = n$)		Refferal to firm ($a = f$)	
	(2)	(3)	(2)	(3)
Constant	2.2207*** (.2998)	2.0837*** (.3811)	.2347*** (.1063)	-.1196 (.1054)
R_{ij}	.1432*** (.0238)	.1237*** (.0185)	.0784*** (.0241)	.0605*** (.0182)
male	-.4281*** (.0322)	-.3378*** (.0204)	.0266*** (.0068)	.2031*** (.0094)
male x R_{ij}	-.0047 (.0205)	.0457*** (.0138)	-.0065 (.0152)	.0426*** (.0076)
Age 15-24	.1423*** (.0539)	.4552*** (.0550)	-.0300** (.0119)	.0173 (.0125)
15-24 x R_{ij}	.0018 (.0298)	.0482 (.0493)	-.0030 (.0176)	.0738*** (.0263)
Age 25-34	-.1018*** (.0381)	-.1012*** (.0371)	-.0204** (.0093)	.0513*** (.0161)
25-34 x R_{ij}	.0256 (.0182)	.0245 (.0264)	-.0030 (.0176)	.0072 (.0183)
Age 55-65	.2559*** (.0414)	.5401*** (.0379)	.0471*** (.0109)	.1673*** (.0119)
55-65 x R_{ij}	.0253	.0382	-.0268	.0203

	(.0310)	(.0344)	(.0109)	(.0213)
Unskilled	.2130*** (.0485)	.7077*** (.0436)	.1592*** (.0117)	.4913*** (.0232)
Uskill x R_{ij}	.0106 (.0224)	.1874*** (.0389)	.0136 (.0108)	.1640*** (.0283)
Medium Skilled	.0335 (.0370)	.1684*** (.0195)	.1085*** (.0069)	.1859*** (.0064)
Mskill x R_{ij}	-.0206 (.0193)	.0199 (.0158)	.0122 (.0127)	.0353*** (.0118)
Highskilled	-.3452*** (.0656)	-.1757** (.0806)	.0887*** (.0310)	.3355*** (.0601)
Hskill x R_{ij}	.1225 (.1632)	.7734 (.7215)	.1639 (.1705)	.8573 (.7615)
Greek	.0714 (.1938)	.6593** (.3124)	.0544 (.0442)	.6890*** (.1534)
Greek x R_{ij}	.2102** (.0924)	1.1252*** (.3955)	.0544 (.0442)	.9341*** (.3205)
Italian	.3245** (.1563)	.8777*** (.2147)	.0388 (.0508)	.4942*** (.1168)
Italian x R_{ij}	.2061 (.2092)	1.3011** (.5590)	.2183 (.2092)	.9314*** (.2962)
Spanish	.4197 (.4024)	.2624 (.5732)	.0565 (.1050)	.5697* (.3239)
Spanish x R_{ij}	-.1935 (.1337)	1.0928 (1.0306)	.0036 (.0508)	.4120 (.7877)
Turkish	.1791** (.0793)	1.0417*** (.1300)	.1543*** (.0247)	.9615*** (.0911)
Turkish x R_{ij}	.0355 (.0404)	.1888 (.1221)	.0392** (.0160)	.1672** (.0677)
Yugoslavian ^a	.1747 (.1409)	.5328** (.2055)	.0206 (.0309)	.3085*** (.0728)
Yugo. x R_{ij}	.1214* (.0665)	1.0888*** (.2647)	.0012 (.0189)	.6416*** (.1313)
From new EU ^a	-.0339 (.1657)	1.3035* (.6390)	-.0571 (.0384)	.3241 (.3214)
New EU x R_{ij}	.5642* (.3133)	23.8789*** (5.5630)	.4976 (.3144)	23.4559*** (6.9550)
Primary Sector	.1152*	6.1700***	-.2440*	5.0837***

	(.0697)	(.3083)	(.0191)	(.2351)
PSector x R_{ij}	-.0936**	1.777***	-.0268	1.8385***
	(.0373)	(.3862)	(.0373)	(.3411)
Construction	-.1783**	.6681***	-.3861**	.4942***
	(.0593)	(.0993)	(.0181)	(.1169)
Constr. x R_{ij}	-.0293	.7193***	-.0703**	.1385***
	(.0364)	(.1031)	(.0215)	(.0259)
TTC ^b	.3323***	.7939***	-.3072***	.4300***
	(.0434)	(.0336)	(.0173)	(.0163)
TTC x R_{ij}	-.0493*	.2150***	-.0661*	.1827***
	(.0279)	(.0356)	(.0205)	(.0321)
Buisness Services	.3367***	1.1018***	.3367***	1.1018***
	(.0530)	(.0563)	(.0530)	(.0563)
Buisness x R_{ij}	-.0996***	.1754***	-.0996***	.1754***
	(.0304)	(.0404)	(.0304)	(.0404)
Other Service	.6980***	1.7521***	-.1078***	.8796***
	(.0120)	(.1761)	(.0205)	(.0307)
Services x R_{ij}	-.0171	.03045*	.0112	.3091*
	(.0622)	(.1761)	(.0613)	(.1853)
coresize	32.5784	27.6795	-4.3344	-3.9490
	(20.6601)	(19.6010)	(5.4710)	(5.4285)
csize x R_{ij}	-.0822***	-.0882***	-.0500***	-.0571***
	(.0120)	(.0124)	(.0072)	(.0083)
σ_u	36.6795	31.1610	5.1042	4.6731
σ_ε	13.2821	13.2703	4.7623	4.7485
# pairs	179.7 Mio	179.7 Mio	179.7 Mio	179.7 Mio
# groups	10,159	10,159	10,159	10,159
Corr(u,Xb)	-.0042	-.9996	.0249	-.9984

Heteroscedasticity-consistent standard errors in parentheses. */**/** mark significance at the 90%/95%/99% confidence level. ^a: Yugoslavian covers immigrants from the territory of former Yugoslavia (including Slovenia and Croatia); these are not included in the group of immigrants from new EU members (which come from Estonia, Latvia, Lithuania, Poland, Czech Republic, Slovakia, Hungary, Bulgaria, Romania, Malta and Cyprus).

^b: Trade, Transportation and Communication (TTC).

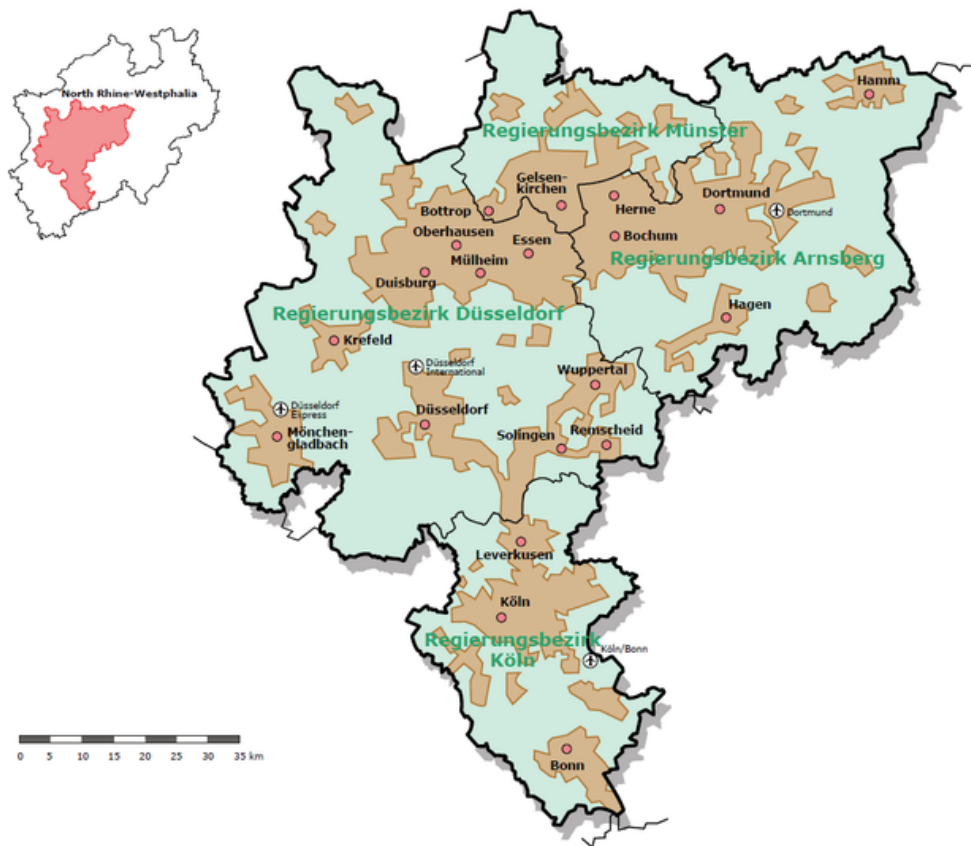


FIGURE B.1: Rhein-Ruhr Metropolitan Area

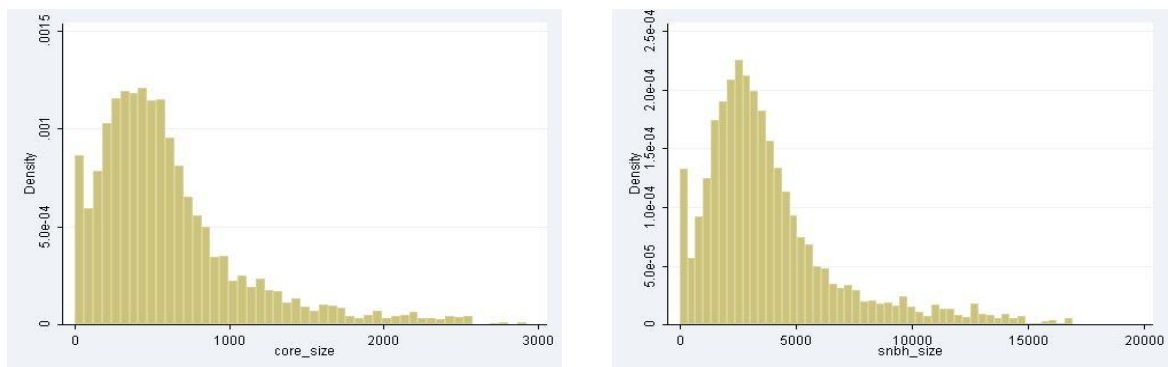


FIGURE B.2: Size distribution of neighborhoods and super-neighborhoods

TABLE B.1: Correlation between individual and average characteristics across neighbors

	Within neighborhood		Super-neighborhood, without neighborhood	
	unconditional	cond. on neighborhood	unconditional	cond. on neighborhood
female	0.4919	0.4920	0.4954	0.4955
male	0.5062	0.5060	0.5050	0.5048
Age 15-24	0.1153	0.1235	0.1101	0.1145
Age 25-34	0.2038	0.2071	0.1949	0.1953
Age 35-54	0.5288	0.5254	0.5266	0.5255
Age 55-65	0.1739	0.1764	0.1720	0.1730
Unskilled	0.1267	0.1340	0.1226	0.1244
Med. skilled	0.3958	0.3913	0.4024	0.4012
Highskilled	0.1182	0.1074	0.1104	0.1014
Unknown skill	0.2611	0.2684	0.2576	0.2618
German	0.8766	0.8731	0.8830	0.8817
Greek	0.0042	-0.0210	0.0062	-0.0229
Italian	0.0082	0.0188	0.0053	-0.0068
Spanish	0.0041	-0.0577	0.0024	-0.0373
Turkish	0.0623	0.1157	0.0481	0.0817
Yugoslavian ^a	0.0254	0.0626	0.0161	0.0257
From new EU ^a	0.0159	0.0787	0.0089	0.0336
Other nationality	0.0687	0.0933	0.0538	0.0706
Primary sector	0.0208	0.0177	0.0241	0.0200
Manufacturing	0.1372	0.1349	0.1352	0.1337
Construction	0.0288	0.0283	0.0288	0.0252
TTC ^b	0.2595	0.2612	0.2604	0.2619
Business Services	0.1672	0.1715	0.1687	0.1706
Other Services	0.2326	0.2287	0.2365	0.2346

^a: Yugoslavian covers immigrants from the territory of former Yugoslavia (including Slovenia and Croatia); these are not included in the group of immigrants from new EU members (which come from Estonia, Latvia, Lithuania, Poland, Czech Republic, Slovakia, Hungary, Bulgaria, Romania, Malta and Cyprus).

^b: Trade, Transportation and Communication (TTC).

Appendix C

The Distribution of Commuting Subsidies

Deriving Individual Deductible Amounts and Tax Savings from Administrative Data

Drawing on the exact geo-coordinates contained in the IEB data and using ArcGIS, we first calculate the precise road distance between each worker's place of living and place of work. The resulting worker-specific commuting distances allow for determining individual tax breaks for commuting. We calculate the amount of commuting expenses that each worker can legally offset against tax according to the following procedure.

For the years 2004 to 2006, we assign the deductible lump-sum of 920 Euro to all workers commuting less than 14 kilometers per way. For all workers with commuting distances above this threshold, we calculate the deductible amount as

$$DeductibleAmount_i^{2004-06} = CommutingDistance_i \times 0.30 \times 223 \quad (C.1)$$

where 0.30 Euro is the deductible amount per kilometer of a one-way commute and 223 is the average number of working days during the period of observation (365 calendar days - 104 weekend days - 9 public holidays - 29 vacation days (see ?)).

After 2007, we apply a threshold of 34 kilometers for the lump-sum deduction. For distances above this threshold, we use the adjusted formula

$$DeductibleAmount_i^{2007-08} = (CommutingDistance_i - 20) \times 0.30 \times 223 \quad (C.2)$$

where we account for the fact that commuting costs for the first 20 kilometers cannot be offset against tax anymore.

Translating individual deductible amounts into individual tax savings requires information on each worker's marginal tax rate. In order to derive marginal tax rates we follow the procedure by Günselmann (2014), to which we add precise information on commuting distances. Despite some simplifying assumptions required for lack of precise information, Reichert (2014) shows that net wages calculated from administrative data using this procedure deviate from net wages contained in representative survey data (Panel Study Labor Market and Social Security, PASS) by only 6 Euro, suggesting that the size of measurement error is small.

Daily gross wages, which are contained in the IEB, provide the point of departure. Multiplying these with the number of calendar days per year yields the annual gross wage of each worker (see ?). From gross wages, workers can deduct costs for mandatory and voluntary insurances (mainly health, pension, and unemployment), for special expenses (e.g., costs for illness or disability that are not covered by the health insurance and exceed a pre-defined share of individual gross income (one to seven percent, depending on income and number of children)), and for income related expenses (see Doerrenberg et al. (forthcoming)). Regarding the first two categories, we deduct the lump-sums for social insurance contributions (*Vorsorgepauschale*) and for special expenses (*Sonderausgaben-Pauschbetrag*) as they are defined in the German Income Tax Act (*Einkommensteuergesetz*). In addition, we offset the worker-specific tax break on commuting costs we have calculated before.

This procedure yields the individual taxable income, from which individual tax rates can be determined. In contrast to other national tax systems, the marginal tax rate in Germany is not clustered in brackets but rises linearly with individual taxable income (ITI). The tax due T is calculated according to the formula

$$T = \begin{cases} 0 & \text{for } ITI \leq 7,664 \\ (883.7 \times \frac{ITI-7,664}{10,000} + 1,500) \times \frac{ITI-7,664}{10,000} & \text{for } 7,664 < ITI \leq 12,739 \\ (228.7 \times \frac{ITI-12,739}{10,000} + 2,397) \times \frac{ITI-12,739}{10,000} + 989 & \text{for } 12,739 < ITI \leq 52,151 \\ 0.42 \times ITI - 7,914 & \text{for } 52,151 < ITI \leq 250,000 \\ 0.45 \times ITI - 15,414 & \text{for } ITI > 250,000 \end{cases}$$

The ratio between the tax due T and each worker's gross wage yields the individual tax rate. Multiplying this with the individual deductible amount for commuting costs provides the annual net wage effect that workers incur from the existence of tax breaks on commuting costs.

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